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THE QUARTERLY REVIEW *of* BIOLOGY

BIOLOGICAL PROBLEMS AND IMMUNOCHEMISTRY

By FELIX HAUROWITZ

Department of Chemistry, Indiana University

IMMUNOLOGY as a branch of biology and medicine started with the observation that human and animal organisms which had survived bacterial infections became immune to a later infection of the same kind. It was soon found that the serum of the immune organism is able to agglutinate the respective microorganisms, to lyse them or to render possible their phagocytosis. These actions of the immune serum are directed specifically against the microorganism whose invasion had been previously overcome.

The unknown substance of the immune serum which reacts with the invading bacteria was called *antibody*, while the name *antigen* was chosen for the foreign agent which induced the formation of antibodies. This term antigen is not very suitable, however, because antigens, generally, are not directed against genes. The antigens used in older immunological work were bacteria and cells, chiefly red blood cells. Later, the important discovery was made that also uniform substances, such as crystalline proteins, are able to act as antigens, and that the antigenicity of cellular antigens is not due to the cell as a whole, but to the presence of several antigenic substances in the cell. Accordingly, each cell has to be considered as a *mosaic of different antigens* giving rise to the formation of a variety of antibodies. Thus, the injection of pneumococci into rabbits leads to the formation of antibodies against the species-specific protein of pneumococci and of other antibodies against the type-specific capsular polysaccharides (Heidelberger and Avery, 1924). Both types of antibodies can be separated from each other A multiplicity of different antigens has also been found in red blood cells. Human erythrocytes contain the important blood group substances A and B. They are polysaccharides of the same type as those found in certain bacteria (Finland and Curnen, 1940; Kabat et al., 1948) and have also been found in saliva and in the gastric juice. The chemical nature of the M, N, and Rh antigens of human erythrocytes has not yet been elucidated. A still larger mosaic of different antigens has been found in the red blood cells of the dove (Irwin, 1947). The great importance of all these antigens for problems of anthropology, systematics, and genetics is discussed in the following papers, by Professors Boyd and Irwin, respectively, who have contributed signally to our knowledge about these things.

If we want to get an insight into the mechanism of immunity, it is preferable to use uniform chemical substances as antigens. These are not only simpler than the complex cellular antigens, but can also be determined accurately by means of chemical analytical methods, and can be measured in the usual units of weight. The use of chemical substances as antigens has given rise to a new branch of immunology, to immuno-chemistry. It is obvious that this is the part of immunology from which essential contributions concerning the nature of immunological reactions can be expected. It is the task of this communication to report on advances in this field.

The technique most frequently used in immuno-

chemistry consists of repeated injections of an antigen into rabbits or horses. The injections have to be made at short intervals, of 1 to 3 days, and are repeated until the serum has a high titer of antibodies. The antibody titer is determined by the precipitin test. This test is based on the fact that the antibody and the homologous antigen give a precipitate, if mixed in suitable proportions. Since the precipitate is soluble in an excess of the antigen and, sometimes, also in an excess of antibody, it is necessary to prepare a series of different proportions of antigen and antibody. If a quantitative analysis is wanted, the maximum precipitate is centrifuged, washed, and then analyzed for its content of antigen and of antibody. The antibody content of the immune serum can then be expressed in weight units, e.g., in mg. per cent.

The precipitating antibodies were called precipitins, and it was originally believed that they were different from agglutinins, lysins, opsonins, and other types of antibodies. Later it was found, however, that antibodies produced by the injection of pneumococcus polysaccharides are not only able to precipitate those polysaccharides, but can also agglutinate pneumococci and protect animals against virulent pneumococci (Chow et al., 1937; Heidelberger and Kabat, 1936). It is evident, therefore, that precipitation, agglutination, and other immunological reactions are different manifestations of the same antibody, varying according to the physical state of the substrate.

While rabbits and horses produce large amounts of soluble antibodies detectable in the blood serum, other animals, such as the guinea-pig, contain hardly any demonstrable antibody in the serum. Nevertheless, these animals react with severe symptoms upon the reinjection of a previously injected antigen. Their specific reaction is due to certain cells of the connective tissue, which form the lymphoid-macrophage system (Taliaferro and Mulligan, 1937). Intravenous injection of the antigen provokes a heavy shock, called anaphylactic shock, while the local application leads to local destructions of tissues and to necrosis. It is evident from these manifestations that the presence of antibodies does not always have favorable effects. The term immunization has been replaced by the term sensitization in such cases. But both phenomena have the same basis, namely the presence of antibodies. The term allergy is sometimes used for both states of abnormal reactivity, for immunization as well as for sensitization.

THE CHEMICAL NATURE OF ANTIGENS

The first problem to be solved by immunochemical research was the problem whether the antigenicity of proteins or of other uniform antigens is due to the antigen molecule as an entity, or only to certain parts of the molecule. It was proved by K. Landsteiner in his fundamental work that *only certain parts of the antigen molecule are responsible for its specificity*. Landsteiner coupled proteins with small molecules of known constitution and used these complex protein compounds as antigens. The method preferentially used for coupling was diazotization, because it can be performed under very mild conditions, namely, by mixing protein and diazo compound in an aqueous solution at a pH of 8 to 9 at 0°C (Landsteiner and Lampl, 1918). The procedure used by Landsteiner and, later, by many other authors is illustrated by the following example. Horse serum protein is coupled with diazotized sulfanilic acid and is injected into rabbits. Several weeks later the serum of the rabbit is tested with an azoprotein prepared from chicken serum protein and sulfanilic acid. The positive precipitin test shows that the antibodies formed in response to horse serum protein-phenyl-sulfonic acid react also with chicken serum protein-phenyl-sulfonic acid. Since the two proteins used, horse serum protein and chicken serum protein, are quite different from each other serologically, the reaction of the antiserum with both of them is certainly due to the presence of p-phenylsulfonic acid groups in both azoproteins. The specificity of this reaction is astonishingly high. Thus, antibodies against p-phenylsulfonic acid groups are not able to react with o- or with m-phenylsulfonic acid groups (Landsteiner, 1946). Likewise dextro- and laevo-compounds can be differentiated from each other by their respective antibodies (Landsteiner, 1946). It was later found that the strong antigenic property of the phenylsulfonic acid groups is due to their ionization, i.e., to their negative electrical charge. Also, other strongly polar groups, such as the carboxy groups, the arsonic acid group, or the strongly basic quaternary ammonium group are antigenic (Haurowitz, 1942). These groups of the antigen molecule which determine the specificity of the antibodies produced are called *determinant groups* (Doerr, 1929).

Simple chemical compounds, such as sulfanilic acid, are not able to induce the formation of antibodies unless they are bound to a *protein carrier*. Apparently the function of the carrier is to convey the determinant group to the site of antibody formation. Some simple compounds such as, for instance, iodine or certain nitro compounds, are able to act directly as antigens. This is due to the fact that these substances combine readily with proteins of the organism injected to form an efficient antigen (Landsteiner and Jacobs, 1936; Wormall, 1930; Gell, Harington, and Pitt Rivers, 1946).

Usually the natural antigens contain more than one kind of determinant group. Likewise, more than one kind of determinant group can be introduced artificially into the molecule of protein carriers. Thus, I have prepared complex antigens containing the determinant p-phenylarsonic acid group as well as the di-iodophenol group (Haurowitz and Schwerin, 1943). Rabbits injected with this complex antigen produced two different antibodies against these two different determinant groups. Generalizing, we may say that the injection of an antigen containing the determinant groups, A, B, C, ··· Z, induces the formation of antibodies of the types anti-A, anti-B, anti-C,··· anti-Z. This situation is complicated by the fact that antibodies are formed which react with 2 or more determinant groups, for example, type anti-AB. Since antigen molecules, even if they are uniform, contain a multiplicity of determinant groups, *each immune serum contains a multiplicity of different antibodies.*

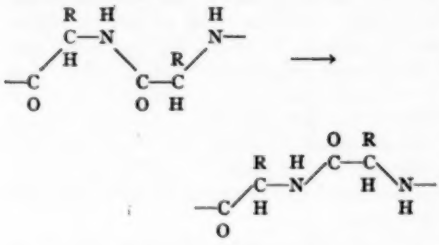
ANTIBODIES

We must now raise the main question of immunology—the question: "What are antibodies?". To answer this question, we have to analyze the precipitates or the pure antibodies which can be recovered from the precipitates (Felton, 1932; Heidelberger, Pedersen, and Tiselius, 1936; Haurowitz, Tekman, Bilen, and Schwerin, 1947). Such analyses reveal that *antibodies are proteins*, very similar to the gamma-globulins of the blood serum. The gamma-globulins are slightly more alkaline than the alpha- and beta-globulins of the serum. They are precipitated by one-third saturation with ammonium sulphate, while most of the other globulins require one-half saturation for their precipitation (Tiselius, 1937). The amino acid composition of the antibodies and their physical-chemical properties are hardly different from those of normal gamma-globulins. The molecular weight is in some antibodies equal to that of normal gamma-globulins; in other antibodies it is higher (Heidelberger, Pedersen, and Tiselius, 1936). But it is evident that there must be some difference between the structure of antibodies and that of normal gamma-globulins. To understand this difference, it is necessary to review, briefly, our present knowledge about the structure of protein molecules.

We know that protein molecules consist of long chains of amino acids linked to one another by peptide bonds. Since the average molecular weight of serum globulins is approximately 175,000 and that of an amino acid residue approximately 115, each globulin molecule contains about 1500 amino acid molecules. Since we have to deal with about 20 different types of amino acids, an endless number of combinations is possible. Hitherto, however, no noticeable differences in the amino acid composition of antibodies and of normal serum globulins have been found. Most probably the differences between antibodies and normal serum globulins are due to differences in the mode of folding of the peptide chains (Pauling, 1940). The length of a peptide chain formed by 1500 amino acids amounts to 550 millimicrons. On the other hand, it has been proved by measurements of viscosity and of flow-birefringence that globulin molecules are ellipsoidal molecules whose longitudinal and transversal axes have a length of 23.5 and 4.4 millimicrons, respectively (Edsall, 1947). It is evident, therefore, that the peptide chain is repeatedly folded to fit the above-mentioned dimensions of the molecule.

In order to explain the specificity of antibodies and the combination of antibodies with their antigens, I advanced, together with Breinl (Breinl and Haurowitz, 1930; Haurowitz, 1947a) the idea that *the surfaces of antibody molecules are adapted structurally to the determinant groups of the homologous antigen molecules.* Similar views were advanced independently by Mudd (1932) and by Alexander (1931). The structural adaptation can be brought about by deformations of the peptide chains. The deformability of these chains can easily be demonstrated in molecular models. It is due to the fact that each part of the peptide chain can freely rotate around the longi-

tudinal axes of the —C—C—N—C—C—N— bonds, as follows:



Although free rotation is possible in peptide chains, it cannot take place in globular protein molecules, for it has been proved by x-ray interferences and by the high density of globular proteins that their peptide chains are folded very tightly. Even the small water molecules are not able to penetrate between the peptide chains of protein crystals (Boyes-Watson, Davidson, and Perutz, 1947). We have to postulate, therefore, that *the complementary adaptation of antibodies to the determinant groups of the antigen molecule must take place* in statu nascenti, i.e., *during the formation of the antibody molecule.* By denaturation, for instance by heat or by the action of acids or bases, we can destroy the specific arrangement of the peptide chains. At the same time, however, we destroy the antibody character of the antibody molecule. It is clear from these facts that the *essential feature of antibodies is the appropriate folding of their peptide chains* (Pauling, 1940), *by which the complementariness of their surfaces to that of the antigen molecule is achieved* (Breinl and Haurowitz, 1930; Haurowitz, 1947a). Although this complementariness of the antibody molecule can be considerable, it can never be complete, for the distances between the C and N atoms of the main peptide chain and the angles between these atoms are inalterable. Therefore, the structural adaptation of the antibody molecule will be more or less incomplete. Our experiments show that each immune serum contains antibodies of a different degree of adaptation. While the poorly adapted antibodies are bound only by the homologous antigen, the well adapted antibodies are also bound by those test antigens which possess only one type of the determinant groups (Haurowitz, 1942).

THE REACTION BETWEEN ANTIGEN AND ANTIBODY

The reaction taking place between antigen and antibody can be analyzed quantitatively, if antigens are used which can be differentiated readily from the antibody molecules. Thus, colored antigens such as hemoglobin, hemocyanin, or the red azoproteins can be determined by colorimetry. Iodoproteins or polysaccharides are determined by the determination of iodine or of their sugars, respectively. Quantitative analyses reveal that the antibody/antigen ratio in the precipitates is not constant. It depends chiefly on the concentration of antigen and antibody in their solutions and decreases if increasing amounts of antigen are added to a certain amount of antibody (Marrack, 1938; Heidelberger, 1939; Boyd, 1943; Haurowitz, 1939). This is due to the fact that the antigen molecules contain more than one determinant group, so that a large number of antibody molecules is attached to each antigen molecule if an excess of antibody is present. If, however, an excess of the antigen is added, not all determinant groups of the multivalent antigen molecules will be occupied by antibody molecules, so that small antigen-antibody complexes will result. While the large complexes formed in the presence of an excess of antibody are insoluble, the small complexes formed in antigen excess are soluble. The antibody/antigen ratio in the precipitate depends also on the size of the antigen particles (Boyd and Hooker, 1934), because the antibody molecules can not diffuse into the interior of the antigen particle and are linked only to its surface. When cellular antigens, such as bacteria or red blood cells are used, the antibody/antigen ratio will be very small, because the antibody forms a negligibly thin film on the surface of the cellular particle. On the other hand, if small protein molecules, such as ovalbumin, are precipitated by an immune serum, each of the antigen molecules will combine with several antibody molecules, and the antibody content of the precipitate will be 80 per cent or more of the total weight of the precipitate (Boyd and Hooker, 1934; Marrack, 1942).

Since most antigens contain a multiplicity of different determinant groups and since immune sera contain different types of antibodies, each of them represented by more or less perfectly adapted molecules, the reaction taking place between antigen and antibody is a very complex reaction, even if uniform antigens are used. It is very difficult, therefore, to determine the content of an immune serum in different antibodies; but that an approximate determination of two or more kinds of antibody in an immune serum is possible is shown by Fig. 1.

The figure shows the determination of antibodies in an immune serum obtained by the injection of arsanil-azo-sheep serum globulin into a rabbit. This azoprotein is prepared by coupling diazotized arsanilic acid with sheep serum globulin. Only those antibodies which are adapted to the phenylarsonic acid group to a high degree are precipitated by added arsanil-azoovalbumin. The immune serum, after exhaustion with this first test antigen, gives further precipitates upon the addition of the homologous arsanil-sheep serum globulin. The antibodies precipitated by this second test antigen

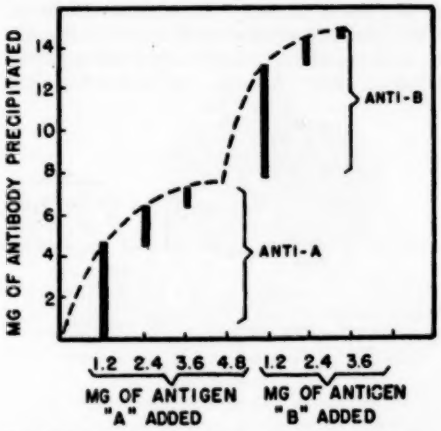


FIG. 1. PRECIPITATION OF 3 ML. OF AN IMMUNE SERUM TO ARSANIL-SHEEP SERUM GLOBULIN BY THE FRACTIONAL ADDITION OF (A) ARSANIL-OVALBUMIN AND (B) ARSANIL-SHEEP SERUM GLOBULIN IN DOSES OF 1.2 MG.

The vertical columns show the amounts of antibody precipitated.

are poorly adapted to the phenylarsonic acid group, so that they are not precipitated by the heterologous azo-ovalbumin. They are precipitated by the homologous azo-sheep serum globulin, because this possesses also the species-specific determinant groups of sheep serum globulin.

The question arises whether the complicated reactions taking place in the sensitized organism can be explained simply by a combination of antigen and antibody particles. This seems to be the case. Toxic antigens lose their toxicity even in vitro if they combine with antitoxins. Antibodies against bacteria or cells are fixed to the surface of these particles and deprive them, in this way, of their electric charges, which are indispensable for the mutual repulsion of these particles and for the stability of their suspensions. Cells which are coated with a layer of antibody molecules stick together and agglutinate. They lose at the same time their resistance to lytic enzymes and to phagocytes, so that they undergo lysis and phagocytosis.

If we attribute the mutual combination of antigen and antibody particles to the mutual complementary adaptation of their surfaces, we have to explain why complementarily adapted particles are bound to each other. This is mainly due to the electrostatic forces of polar groups present on the surface of antigen and antibody particles. Protein molecules possess a large number of polar atomic groups such as hydroxy, amino, or carboxy groups, and the acid amide groups of the peptide bonds. Also, the carbohydrates are rich in polar groups, mainly hydroxy groups. Another polar molecule, which is always present in biological systems, is the water molecule. According to the laws of physics, polar groups attract each other by electrostatic forces. These forces are short-range forces. Their power decreases with the sixth power of the reciprocal distance. Accordingly, they operate only between closely adjacent molecules. In biological systems, forces of attraction will be effective between protein, carbohydrate, and water molecules. In this competition, the water molecules will be most successful, because they are very small and can come very close to the polar groups of the protein or carbohydrate molecules, a phenomenon well known as hydration. Hydration prevents the mutual combination of protein molecules in their aqueous solutions. If, however, the surfaces of two particles are adapted complementarily to each other, large parts of their surfaces can approach each other very closely and the power of attraction increases to such a degree that the particles remain bound to each other after a suitable collision of their surfaces. We can compare the forces operating between complementarily shaped particles with those exerted by a layer of glue covering two wooden blocks. Only if large areas of the wooden blocks are complementarily adapted to each other will they stick firmly to each other.

PROTEIN SYNTHESIS AND THE FORMATION OF ANTIBODIES

The formation of antibodies and their complementary adaptation to the antigen molecules are of the greatest importance, not only for immunology, but for biology in general, for antibodies are

proteins and *antibody synthesis is protein synthesis.* The synthesis of proteins in the living organism can hardly be modified by experimental interference. Even if we alter drastically the composition of food or if we starve an animal, it will continue to produce its normal cellular proteins, but, in the injection of antigens, immunochemistry furnishes us a tool by which we are able to alter the normal synthesis of proteins in a definite and reproducible manner so that the shape of the protein formed becomes adapted to the antigen molecule.

One can hardly suppose that this kind of protein formation, the formation of a complementarily adapted negative replica, is restricted to the formation of antibodies. It seems necessary to assume an analogous mechanism for the formation of normal proteins. Actually, Tyler (1948) has recently advanced the idea that normal globulins are complementary to substances comprising the normal site of globulin synthesis and has proposed the designation "auto-antibodies." The concept of the analogous formation of antibodies and of normal proteins is supported by the fact that intermediates between antibodies and normal globulins have been found (Haurowitz and Cindi, 1946) and that no sharp borderline between antibodies and normal globulins can be drawn.

If we accept the view that the normal cellular proteins are formed as negative replicas of a positive cellular template, we are confronted with a serious dilemma, for there is no doubt that some proteins, such as those of the genes, of viruses, and other similar units of the living substance, are formed by self-reproduction, i.e., as *positive replicas* of their templates. The idea that protein synthesis could take place by formation of positive *and* of negative replicas at the same time and in the same organism is so unlikely that it must be rejected. Both ideas, however, can be reconciled with each other if we realize that *the formation of a protein molecule must take place in two successive steps.*

The first of these steps is the formation of the specific peptide chain containing a definite number of each of the amino acids arranged in a definite order. One has to assume that *this first phase of the protein synthesis takes place in an expanded monomolecular layer* of the peptide chains (Muller, 1922; Eyring, Johnson, and Gensler, 1946), for the intermolecular forces to which this synthesis must be ascribed are short-range forces which cannot operate over distances of more than 0.3 to 0.5 millimicrons. This is less than the transverse diameter of a peptide chain. It is evident that these short-range forces of the template molecule cannot have any influence on remote parts of the globular molecule formed and cannot be responsible for its 3-dimensional structure. The formation of the complicated pattern of the expanded peptide chain is only understandable if we assume that the same pattern has served as template. We find an analogy in the growth of crystals. There, too, monolayers of identical daughter molecules are deposited on the surface of the parent cyrstal. The high specificity of crystallization has been compared with the specificity of biological reactions and of antibody synthesis (Pauling, 1948). Actually, only molecules of the

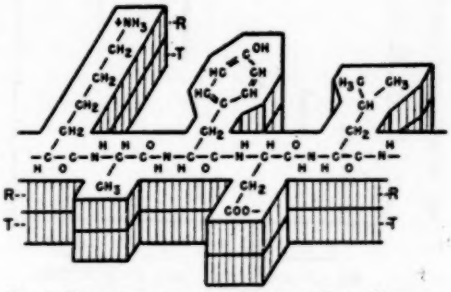


FIG. 2. REPRODUCTION OF A PEPTIDE CHAIN FORMED BY LYSINE, ALANINE, TYROSINE, ASPARTIC ACID, AND LEUCINE

T = template, R = replica

same kind as those which form the crystal are laid down on its surface. The same may be true for the formation of protein replicas. We can imagine that a 2-dimensional peptide monolayer, covered by a solution containing different amino acids, will preferentially adsorb amino acids of the same kind, so that tyrosine, leucine, and alanine will be adsorbed to tyrosyl, leucyl, or alanyl residues, respectively, of the expanded peptide chains (Delbrück, 1941). If these adsorbed amino acids, in the presence of a non-specific enzyme, undergo condensation, a positive two-dimensional replica of the expanded peptide layer will be formed (Fig. 2).

The second phase of protein synthesis is the folding of the two-dimensional replica, so that a three-dimensional globular molecule is formed. The mode of folding will depend on the constitution and the order of the amino acids forming the peptide

chain, on the one hand, and on the specific arrangement of the adjacent polar molecules, on the other. The strongest attraction on the peptide layer formed will be exerted by polar groups of adjacent molecules or of the expanded template layer. Accordingly, the replica will adhere to these polar groups during the process of folding, and that part of the globular molecule formed, which is fixed by these polar groups, will acquire a complementary shape and will thus become a negative replica of the polar groups (Fig. 3). Since a globular molecule occupies only 5 to 10 per cent of the area occupied by the same molecule in the expanded state, the complementarily shaped part of the globular molecule forms only a small part of the whole surface of the folded particle.

FIG. 3. SECOND STEP OF PROTEIN SYNTHESIS.
Black area, cross section of particle which functions as template in second step of protein synthesis. P = protein.

The same considerations can be applied to the formation of antibodies. It has been proved that the bulk of the injected antigen is deposited in the liver and the bone-marrow, i.e., in those organs where large amounts of protein are formed (Haurowitz and Breinl, 1932). We have to assume that the antigen molecule is deposited there in the native state, for the specificity of the antigen is destroyed as soon as the molecule is unfolded and thus denatured. The first phase of protein synthesis, the formation of the 2-dimensional positive replica, will hardly be disturbed by the globular antigen particle, because this can occupy only a small part of the expanded protein templates. But if the folding of the replica takes place on the surface of the deposited antigen particle, the 3-dimensional replica will acquire a shape which is a negative print of the determinant parts of the antigen molecule. The polar particle which acts as template in the normal cell and which is represented in Fig. 3 by the black area, is then replaced by the antigen particle, so that Fig. 3 represents at the same time the formation of an antibody molecule.

It is an advantage of the new picture of protein synthesis that it comprises the formation of normal proteins as well as that of antibodies, and thus reconciles the contradictory assumptions of positive or negative replicas, or both. This picture would, however, be incomplete if account were not taken of the important role played by the nucleic acids in the synthesis of proteins. Their importance is indicated by the presence of large amounts of ribonucleic acid in the sites of protein formation (Caspersson, 1947; Brachet, 1947; Boivin, 1947). *Nucleic acids* form insoluble compounds with proteins. Their function is, most probably, to render insoluble the 2-dimensional peptide layer, which forms the template for the formation of the 2-dimensional replica. It is obvious that a template has to be rigid and insoluble. A soluble template can hardly be imagined. On the other hand, the expanded protein template and its replica are identical molecules. The fact that only one of them undergoes folding, while the other remains in the expanded state, is satisfactorily explained by the combination of nucleic acid.

IMMUNIZATION AND MUTATION

While many immunological phenomena can be explained by the new concept on the formation of proteins and of antibodies, it must be admitted that there are phenomena which cannot yet be explained to our satisfaction. One of them is the long persistence of antibodies in the immune organism. Immunity against some infectious diseases lasts many years. It has been claimed that the antigen cannot persist in the living organism over so long a period (Burnet, 1941) and that the formation of antibodies must continue in the absence of the antigen. If this were so, the cells producing the antibody would have to be taken for mutated cells, which transmit their capacity for production of antibodies in the presence of antigen to daughter cells that contain no antigen. While there is no doubt that congenital immunological properties, such as the possession of certain blood groups, are inherited, acquired immunity is never transmitted in higher animals.

Recently, however, some connections between active immunization and hereditary changes have been revealed. One of these is the mutagenic action of mustard gas in *Drosophila* (Auerbach and Robson, 1946). Mustard gas combines in vitro slowly with proteins to give typical mustard-protein antigens, which, when injected into rabbits, induce the formation of antibodies against

the mustard component (Boursnell, Francis, and Wormall, 1946). Since the combination of mustard gas with proteins takes place very slowly, free mustard gas can penetrate deeply into the cells, possibly even into the nucleus. Mustard gas combines, also, with nucleotides to form thiazan derivatives (Young and Campbell, 1947). It is not yet clear whether the mutagenic action of mustard gas has anything to do with its antigenic character.

A second similarly important phenomenon is the production of mutations in *Neurospora* (Emerson, 1944) and especially in *Paramecium* (Sonneborn, 1947) by the respective action of anti-neurospora and anti-paramecium sera of rabbits. By the action of such antisera on paramecia, different hereditary types of paramecium can be produced (Sonneborn, 1947). The nature of the chemical substances in paramecia which act as antigens is not yet known, so that we are not yet able to discuss the mutagenic action in terms of chemistry. But it is most remarkable that mutations can be brought about by an antibody-antigen reaction.

We do not yet know whether the mutations induced by mustard gas or by anti-paramecium sera have any relation to the phenomenon of long-lasting immunity in higher animals. If we deny the presence of antigens in these cases of long-lasting immunity, we are compelled to assume the presence of some unknown factor, such as a specific enzyme, which produces antibodies even in the absence of any antigen. The obvious objection against such an assumption is that one can hardly imagine the existence of enzymes which form antibodies against phenyl-arsonic acid, phenyl-sulfonic acid, or other artificial products of the chemical industry. It might be more reasonable to assume that antigen particles remain in the organism over many years, just as coal or silica particles are stored in certain cells over many years. In the case of infectious diseases, one has to consider the possibility that small amounts of the virus or of the bacteria multiply continuously in the immunized host.

SUMMARY

(1) Immunological reactions are reactions between antigens and antibodies.

(2) Antigens are substances which are deposited in the sites of protein synthesis and disturb this process.

(3) Protein synthesis takes place in two successive steps. The first of these is the formation of an expanded 2-dimensional replica of the expanded layer of the respective protein. The second step is the folding of this replica to give a 3-dimensional globular ellipsoidal particle.

(4) Antigens interfere mainly with the second phase of the protein synthesis, so that the globular particle formed is adapted complementarily to the determinant groups of the antigen molecule. If this modified protein passes into the blood serum, it is effective there as antibody. If the modified protein remains in the cell, it is responsible for the phenomena of cellular allergy.

(5) The function of nucleic acids in protein synthesis is to render insoluble and rigid the expanded monomolecular peptide layer of the template.

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SYSTEMATICS, EVOLUTION, AND ANTHROPOLOGY IN THE LIGHT OF IMMUNOLOGY

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PROBLEMS of systematics and taxonomy have long had an interest for biologists, and will probably continue to claim their share of attention. In the absence of paleontological evidence, decisions regarding the interrelationships among animals are based upon peculiarities of distribution, development, and upon similarities of structure in forms which exist today. The verdicts about relationships of different authorities on morphology do not always agree and have not always agreed in the past. A criterion of relationship independent of morphology is clearly desirable, and it was therefore a significant contribution when Nuttall (1904) introduced the use of serological methods, which detect chemical rather than morphological differences, into tests of zoological relationship.

Nuttall based his work on the discovery of von Behring (1890) that the serum of an animal which had been immunized against diphtheria was capable, when injected into a fresh animal, of conferring immunity upon the latter, which without the benefit of the use of the immune serum soon died from the effect of the diphtheria toxin it received as a test. Soon afterward von Behring and Kitasato (1890) obtained similar results with tetanus. The protective effects of these sera were attributed to substances called antibodies. These antibodies were called neutralizing antibodies because they have the power of neutralizing toxins. The chief work of Nuttall was based upon the use of precipitins, that is, those antibodies which, when mixed with their antigens, have the power of producing precipitation.

SEROLOGICAL TESTS OF RELATIONSHIP

It seems that Ehrlich (1891) was the first to attribute a rigidly specific character to precipitins, basing his statement on the investigations of Bordet and Myers. Nuttall, however, was led to question this generalization by his observation that although precipitins for various animal bloods were relatively specific, they might nevertheless produce a slight reaction with the bloods of related animals. This led him to begin, in 1901, to collect as many bloods as he could from many different species of animals.

The data obtained by Nuttall and also by Uhlenhuth indicated that the serological similarities between animals were best demonstrated by means of powerful antisera. Nuttall also noted that the precipitin reactions demonstrated that the old world primates were more closely allied to man than were the new world primates, which was exactly in accordance with the opinion expressed by Darwin, based on structural similarities. The conclusion of a great serologist thus agreed with that of the greatest evolutionist.

Nuttall next proceeded to an elaborate examination of the cross-reactions of a large number of different bloods with different antisera. These tests yielded results which on the whole confirmed the views as to zoological relationships which are now considered soundest. It is interesting to consider how it was attempted to measure relationship serologically. Nuttall based all of his measurements upon the amount (volume) of precipitate which was obtained by adding 0.1 cc. of antiserum to 0.5 cc. of a 1 to 100 dilution of homologous blood. By homologous blood was of course meant blood from an animal of the same species which furnished the blood injected into the experimental animal (generally a rabbit), in the first place. Degrees of relationship were determined by the amount of precipitate resulting when heterologous bloods were added in similar amounts. The results of these investigations were summarized in Nuttall's classic book, published in 1904.

Since Nuttall's time a number of technical advances over the method he employed have been made. In the first place, we know that the speed of reaction, and even more important, the total

amount of precipitate, may vary in accordance with the proportion in which the two reagents, antiserum and antigen, are mixed (Boyd, 1947). Thus by mixing an antiserum with its homologous antigen in the wrong proportions and mixing the antiserum with a heterologous antigen in more favorable proportions, one might obtain more precipitate from the heterologous antigen. That this does not in general happen, however, at least in the hands of skilled investigators, is suggested by the reproducibility of the data of Nuttall and by the general correspondence between his conclusions and those of taxonomists in general.

Another consideration is that the volume of the precipitate does not always accurately indicate the amount of precipitate formed. Differences in the tightness with which a precipitate is packed, variations in character of the precipitate, and so on, may all influence the volume. In more modern work a direct determination in the washed precipitate of protein, or at least of protein nitrogen, is carried out. Examples may be seen in the work of Heidelberger (1939), Kabat (1948), and their school. These workers have studied the serological relationships of various microorganisms. Another technique, recommended by Boyden (1942), who has done extensive work on animal relationships, involves the measurement of the increasing opacity of the precipitating mixture as a guide to the degree of development of precipitate.

Only recently have workers begun to take into account the fact that the amount of precipitate and the rate of its production may depend upon the proportion in which the two reagents are mixed. Therefore the only accurate determinations are those carried out on a series of mixtures in different proportions. Heidelberger and his school (1929) have done this. Other workers, such as Breinl and Haurowitz (1930) and Hooker and Boyd (1936), and Boyden and his co-workers (Boyden, 1942) have also employed this method.

An interesting variation on this latter method is presented in a thesis written by Crosby (1940), who measured the volume of precipitate produced from mixtures of antibody and antigen in various proportions and showed that if the amount of antigen be kept constant and the proportion of antiserum increased, then after a certain point the volume of precipitate reaches a maximum. The various maxima for different antigens can be compared, and degrees of relationship between the animals from which they were derived can thus be measured. Other techniques which have been used involve partial absorption of a serum with one antigen. We cannot go into such purely technical aspects of the problem here.

We have next to consider some possible objections to the immunological approach. One of the fundamental difficulties of all serological investigations of taxonomy is this, that practically all of the antisera used are obtained from rabbits, although the reactions of rabbit antisera with antigens from closely or remotely related species may or may not be strictly indicative of taxonomic relationship. Thus several authors have concluded, from tests with rabbit immune sera, that rats and mice are widely distant zoologically, whereas various species of birds such as chickens, pigeons, geese, and so forth are much more closely related to each other. Landsteiner (1945) characterized these conclusions as a case of faulty perspective. To explain this he postulated that the rabbit would recognize relatively small differences in the serum proteins of other rodents—which must be basically similar to its own—whereas in tests of birds antibodies from rabbits would perhaps be produced first and most readily in response to gross differences, and would ignore those small differences which possibly would have been revealed had more closely related animals been used for the production of the antiserum.

In spite of this difficulty, it seems likely that the use of rabbit serum will continue to predominate, since we are limited by a number of practical considerations, such as the size and availability of animals, in the production of immune antisera. But, if we keep in mind the possible faulty perspective which Landsteiner pointed out, there is no reason why much reliable work should not be accomplished by this means. Only, we must make suitable allowance for the taxonomic gap separating the animal used as recipient (the rabbit) and the animal (or other organism) used as the donor or source of antigen.

An interesting question which has never been thoroughly discussed now arises. Which proteins of the living organism are to be considered most indicative of its zoological relationship to other plants or animals? Shall we take hemoglobin, which seems serologically rather similar in various different species, and which always possesses a prosthetic group, heme, which is identical in all species? Or, shall we accept as more significant the serum albumins or serum globulins, which seem

to show much more serological difference from one species to another? To these questions the serologist is unable to give a definite answer, and can only reply that in the long run the decision must rest with the taxonomist. In other words, serological methods, although offering a valuable contribution, are ultimately merely another method of characterizing species and their degree of relationship to each other. They have no more *absolute* validity than the methods of morphology. An interesting point in logic comes up if we attempt to decide which antigens are most significant by comparing our conclusions about relationships with those based upon morphological comparisons, for in such a case our argument may become circular. For clearly, if we define the most valid serological conclusions as those which parallel most nearly those of structural studies, then serology has no independent contribution to make to taxonomy. But the work of Nuttall (1904), Boyden (1942), Irwin and Cumley (1943), and Wilhelmi (1940), to name but a few in the field, leaves little doubt that serology has already made significant contributions to systematics.

When we come to a discussion of the serological properties of the red blood cells, however, we may decide that serological methods offer some advantages—not, to be sure, because they are in themselves superior, but because so much has been found out about the inheritance of the factors they reveal, and because, as we shall easily see, genetically controlled characters are much more valuable for racial and specific characterization than are characters which are not inherited in a definite manner or which are not known to be inherited.

A serological study of microorganisms, where structural studies tell relatively little, has in many cases proved extraordinarily effective in differentiating different strains. The existence of pneumococcus types was first definitely shown by Neufeld and Händel (1909), who found that immune sera which protected mice against certain strains of pneumococci were not effective against other strains, and that these differences were correlated with differences in the reactions in vitro of the sera with the pneumococci. The study of pneumococci has resulted in their classification into a large number of serological types, differing chiefly in the chemical structure of the polysaccharides of the capsule. Other microorganisms, such as streptococci, have been classified by similar methods. The work of Lancefield (1940) stands out in this connection.

Finally, a very significant feature of all methods of serological classification must be mentioned. If an animal is injected repeatedly with certain antigens, as for instance the blood proteins of a human being, the reactions at first obtained will tend to be relatively species-specific, but later sera may show reactivity with less closely related antigens, such as those of the anthropoids or even of various lower mammals. This phenomenon of the broadening of the specificity of serological reactions was observed by Magnus in studies of the relationship of certain grasses, and has been repeatedly observed in later experiments with antigens, antigenic cells, or mixtures of antigens. Wells and Osborne (1916), using the method of anaphylaxis to measure the strength of cross reactions, showed that reasonably pure proteins could give the same effect. In some of my work with Dr. Hooker I was able to show that even the specificity for a single chemical part of the antigenic complex may become broader and that the sera may begin to react with less closely related chemical structures as immunization proceeds. This lessening of specificity with repeated injections must be kept in mind in comparing results obtained with sera drawn from animals in different stages of immunization.

RACIAL AND SPECIES DIFFERENCES IN BLOOD CELL ANTIGENS

Not only taxonomy but also such subjects as anthropology and animal genetics have been influenced by the methods of immunology. The quantitative comparisons with the precipitin reaction were based on degrees of reaction obtained with a series of related antigens. The species was the important variable, and different individuals of any one species gave the same or at any rate very similar results. When we come to compare agglutination reactions obtained with the red blood cells of higher animals, we find that the reactions are usually all-or-none in character (i.e., a given individual either possesses a certain antigen or does not possess it). In this case our quantitative comparisons are between populations, based on the frequency within the various populations of individuals having certain antigens. In this way we are often able to distinguish races within a single species. This technique goes back essentially to the discovery of the human blood groups

by Landsteiner (1901) in 1900. Landsteiner and his pupils discovered that there were two antigens important for the production of the human blood groups. These were designated by the non-committal letters A and B, and it was soon shown that all human beings could be subdivided into one of four classes: the class possessing neither A nor B, designated now as group O; the class possessing A alone, designated as A; the class possessing B alone, designated as group B; and the class possessing both, designated as AB. Subsequent work has shown that these four groups exist in all human races, with few exceptions, but that the proportions in which they occur may differ markedly, and further that these differences often enable a differentiation to be made between different human races.

Subsequent to the discovery of the A, B blood groups, other blood group antigens were discovered in human red cells. The next to be reported were antigens called M, N and P, discovered by Landsteiner and Levine (see Landsteiner, 1945). Later other antigens, which have largely been lost to us because the reagents for detecting them are no longer available, were reported by other workers. In 1940 a new and very important discovery was reported by Landsteiner and Wiener (1940) when they announced the discovery of the Rh blood factor. This factor had probably been observed, although not named, by previous workers in the field. Levine and other workers (1939) discovered that serological incompatibility between mother and child was often the cause of the hitherto unexplained disease called erythroblastosis fetalis, or in other words, hemolytic disease of the newborn. Later, proof that it was often due to Rh incompatibility gave an added interest to the study of this subject, and at the present time it is known that at least 6 genetically determined antigens operate. They have been designated by Wiener as rh (rh negative), Rh_0, Rh_1, Rh_2, rh′, and rh″ (Wiener, 1948). An Rh^z and an Rh^y have also been predicted and found (Race, 1944).

A classification of human races based on the incidence within them of the blood group genes which have been studied so far (the AB, MN, and various Rh antigens) is very instructive, and if we define our races properly we can distinguish a different serological race for each of the major geographical areas (Table 1). We may obtain the following classification, even without going into the finer details of the subgroups of Rh or of other more recently discovered factors. This is a modification and amplification of a classification proposed by Wiener (1945).

TABLE 1

Differences in Blood Group Gene Frequencies in the Six Serological Races of Man

RACE	A ($=A_2+A_1$)	RATIO A_2/A_1	B	M	N	Rh POS.	Rh NEG.	OTHER GENES
1. Early-European...	ca. 0.20	ca. 0.50	0	ca. 0.55	ca. 0.45	ca. 0.40	ca. 0.60	
2. European (Caucasoid).....	ca. 0.15	0.2–0.3	ca. 0.10	ca. 0.55	ca. 0.45	ca. 0.60	ca. 0.40	Rh′
3. African (Negroid).	ca. 0.10	ca.0.3–0.4	0.10–0.20	ca. 0.55?	ca. 0.45	ca. 0.80	ca. 0.20	$A_{1,3}$; Rh_0
4. Asiatic (Mongoloid).....	ca. 0.20	0	0.25–0.30	ca. 0.55	ca. 0.45	1.00	0	Rh^z
5. American Indian..	0–0.50	0	0	0.9–0.85	0.1–0.15	1.00	0	some Rh^z
6. Australoid.....	ca. 0.30	0	0	0.1–0.15	0.9–0.85	1.00	0	

1. Early-European group (hypothetical)—possessing the highest incidence, more than 30 per cent, of the Rh negative type, and probably no group B. A relatively high frequency of the gene Rh′ and A_2. They are represented today by their modern descendants, the Basques. They probably possessed normal frequencies of the genes M and N, that is, as in most of the world, phenotype MM = ca. 25 per cent, MN = 50 per cent, and NN = ca. 25 per cent.

2. The European (Caucasoid) group—possessing the next highest incidence of the Rh negative type (about 13 to 14 per cent) and a relatively high frequency of the genes Rh′ and A_2, with moderate frequencies of other blood group genes. Normal M and N.

3. The African (Negroid) group—possessing the highest incidence of the Rh_0 type, a moderate incidence of the Rh negative type, relatively high

frequency of genes A_2 and the rare intermediate gene for A ($A_{1,2}$) and intermediate Rh genes, with a rather high frequency of gene B. Probably normal M and N.

4. The Asiatic (Mongoloid) group—possessing high frequencies of the gene B, a relatively high frequency of the rare gene Rh^0, fair amounts of the gene A_1, but little, if any of the gene A_2 and the Rh negative gene. Normal M and N.

5. American Indian group—possessing varying (sometimes high, sometimes zero) frequencies of gene A_1, and no A_2, and probably no B or Rh negative genes. Some Rh^0. Low frequency of gene N.

6. The Australoid group—possessing a high frequency of gene A_1, no A_2, no Rh negative gene. High frequency of gene N and consequently low frequency of gene M.

Investigation of the relatives of man, such as the anthropoid apes, shows that the antigens which we find in the red cells in man are not unknown in his relatives, and indeed the degree of similarity observed bears a close relationship to the degree of zoological relationship which is generally considered to exist. Thus in the chimpanzees we find blood group antigens A and O, in the gorillas we find blood group antigens A and B, and so on. It is of some interest to point out in this connection that the Rh antigen was discovered during the course of a series of experiments designed to analyze the antigenic structure of the M factor in various anthropoids.

This analysis does not stop at the anthropoid relatives of man but extends far down into the less closely related animals. Thus, chickens possess a large number of blood group antigens which are inherited in a very definite manner, and cattle also possess large numbers of blood group antigens, the genetic analysis of which has been brilliantly carried out by Irwin (Irwin and Cumley, 1943).

We may pause to consider the taxonomic advantages of blood factors which are inherited in a known way. In the first place, the known mode of inheritance enables us to make comparisons between populations as opposed to comparisons of individuals only, for the frequencies of the genes determining these factors will be population characteristics. In the second place, it enables us to predict quantitatively what will be the result of crossing two different races of a species, and what distribution of genes will result. In the third place, it throws some light on the probable origin (evolution) of the racial differences which we observe at present, and allows us to attempt a historical reconstruction of gene distributions which may have existed in the past but have been modified by subsequent processes of selection, migration, and that random extinction of genes known as genetic drift.

The mathematical-genetical work of biologists such as R. A. Fisher (1930) and Sewall Wright (1940) has shown us what the pattern of evolution of gene frequencies in populations will be. In large populations natural selection is the important agency, and even small differences in the adaptive value of a trait will determine its increase or decrease, survival or disappearance. The raw material is always mutations. In small populations the phenomenon known as genetic drift will operate, and neutral or even advantageous genes may disappear by accident. Natural selection still operates, but is relatively less important. Fisher (1930), Wright (1940), and Haldane (1924) have derived formulas from which we may calculate the rate of change in gene frequencies due to selection, random genetic drift, mutation, or all three forces operating simultaneously.

As a notable attempt at reconstruction of the earlier genetic composition of a population, we may mention P. B. Candela's analysis (1942) of the distribution of blood group B in Europeans, and his rather convincing arguments that the gene B was introduced, into a population originally lacking it, by the Mongol invaders of the fifth to the fifteenth centuries.

CONCLUSION

We may conclude by attempting to estimate what contributions may reasonably be expected in the future, as a result of the application of immunological methods to systematics, evolution, and anthropology. We may anticipate, first, that the degree of relationship of various anthropoids to man will be estimated not only by the cross-reactions which their bloods exhibit with precipitating sera but also by the incidence of genes which are the same as, or similar to, the blood group genes found in human red cells. I believe we may anticipate with some confidence that the old concepts of human races, based on skin color, hair form, shape of the skull, and so forth, will be eventually superseded by concepts based on the distribution of known genes, of which the blood group genes form the first example, although not, let us hope, the last.

We may anticipate that detailed analyses of the blood group antigens in lower species such as cattle will become available, so that definite predictions as to the characteristics of the probable offspring of a given mating may be made, based on a knowledge of linked and correlated characteristics.

We may expect that the systematists of the lower species will incorporate the newer knowledge of serological reactions into their schemes and in some cases will no doubt modify their concepts somewhat as a result. We should not expect, however, that the newer methods will entirely replace the older morphological methods which were first used, and which will probably continue to be used, at least to some extent.

We can be fairly sure that the comparisons will be made more accurate in the future through the application of more accurate analytical methods, chemical and physical, such as those involving photoelectric techniques or the accurate determination of nitrogen or the direct determination of proteins. The prospect of a new era in which quantitative methods, in addition to qualitative methods similar to those previously used, will be frequently employed is already clearly visible.

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IMMUNOLOGICAL STUDIES IN EMBRYOLOGY AND GENETICS

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INTRODUCTION

THE subject of this symposium suggests that immunology and biology are more or less distinct scientific disciplines and that there is need to show how the first may be useful in advancing knowledge of the other. Some years ago attention was called to the close familial relationship existing between immunology and one branch of biology. At the risk of boresome repetition, this relationship might be reviewed briefly. Shortly after the middle of the preceding century, there were two eggs fertilized at slightly different times in a common mother, biology. These eggs developed, respectively, into the sciences of genetics and bacteriology. A somewhat later fertilization of another egg in the same mother resulted in a third progeny, experimental embryology. Furthermore, bacteriology begat a lusty offspring known as immunology, and genetics originally claimed descent from a double fertilization by botany and zoology, although quite recently there is increasing evidence that bacteriology itself is more than a foster parent. Thus the lines of descent of these different disciplines from a common ancestor, biology, are steadily becoming more closely intertwined. (Perhaps some day some curious mind will attempt to calculate the coefficient of inbreeding of each of these disciplines.) As is so often the case among relatives, at least in the human family, acquaintance is necessary to promote and maintain sympathetic understanding. This symposium is a promising sign that steps have already been taken to encourage these relatives to become friendly, and to learn a little of each other's language.

Indeed, certain fundamental questions in both biology and immunology have much in common. Thus, in immunology very little insight has been gained as to how the globulins of the serum, in which all or nearly all the antibodies are found, can be changed specifically during immunization and how this change can be maintained for varying periods. Likewise, in general biology, the basic problem of how a gene reproduces itself is still a major mystery. A current hypothesis of the manner in which antibodies are formed with their well known specificities—the idea of a master molecule or template which Haurowitz has ably presented—has provided the foundation and much of the framework for proposals of the self-duplication of the gene.

Boyd has effectively pointed out that one of the earliest attempts to link the technics of immunology to a general biological problem—other than in bacteriology—was the classical work of Nuttall (1904), who compared the taxonomic relationships of many species of animals with those deduced from similarities of the serum proteins of these species. Incidentally, the relationships among the species based on morphological criteria and on antigenic similarities were in fairly close agreement. Several workers have extended such studies, as Baier, Boyden, Moody, Wolfe and others. The related work in plants, particularly that by Mez and his co-workers, was reviewed several years ago by Chester (1937). In general, these studies showed that in the higher plants as in animals the relationships between species as revealed by serological tests parallel closely those determined by morphological criteria. Special attention should be called to the studies of Wells and his colleagues, who utilized particular proteins of related species of plants in their experiments on protein specificities.

EMBRYOLOGICAL STUDIES

Many years ago, an immunologist friend made the apt remark that one of the earliest known examples of successful immunization was that of the egg cell by the sperm. That is, once a sperm cell has entered an egg cell, other sperms may

reach the surface of the egg but do not gain admittance. This phenomenon was investigated by Lillie (1913, 1919), and led to the demonstration of the presence in the eggs first of sea-urchins, and later by Lillie and other workers in those of a number of other species, of a substance on the surface of the egg called "fertilizin." (Within the knowledge of the reviewer, this substance has not been demonstrated on the egg cells of mammals.) This fertilizin combines with the so-called "antifertilizin" of the spermatozoon, thus simulating an antigen-antibody system. According to Tyler (1942), who has extended the scope of these studies, fertilizin is a part or more probably the whole of the gelatinous coat that is present on the surface of the egg, and seemingly is a protein or glycoprotein. A substance on the surface of the sperm, called "antifertilizin," has been extracted from sperm. One of its properties is that by interaction with fertilizin is a membrane precipitated on the surface of the gelatinous coat of the egg. Antifertilizin is also presumably a protein and, when injected into rabbits, will incite the formation of antibodies capable of agglutinating the sperm of the homologous species.

Not only was antifertilizin found on the surface of the sperm, but Tyler (1942) also obtained it from below the surface of the egg, by first removing the gelatinous coat (fertilizin) and then making extracts of the remainder of the egg. This antifertilizin from the eggs possessed the same properties as that from the sperm; that is, it neutralized the sperm-agglutinating action of fertilizin, agglutinated the intact eggs with the formation of a precipitation membrane on the gelatinous coat of the egg cell, and, when injected into rabbits, engendered an antiserum which agglutinated the sperm.

As Tyler (1942) has stated, "the finding of antifertilizin within the egg means that below the surface of the cell there is present a substance that is essentially an antibody to the surface substance." Tyler (1946) further demonstrated the presence of protective antibodies for its own venom in the serum of the Gila monster. These findings, and observations by others on auto-agglutination under certain conditions of blood cells and bacteria have led Tyler to propose that many mutually complementary substances may be found deeper within the cell. "This view may then be expanded into a general theory of cell structure; namely, that a cell is a mosaic of substances that are mutually complementary (i.e., capable of combining with one another in the manner of antigen and antibody) substances which are actually in combination with one another in the region where they adjoin. The compounds would be represented by the various membranes, such as the cell membrane, nuclear membrane, nucleolar membrane, vacuolar membrane, etc., which in turn keep the complementary substances apart." Tyler has treated this concept of cell structure in considerable detail in a recent paper (1947).

Assuming that this concept might apply to cells in general, a logical inference regarding the specificity of attraction between chromosomes in synapsis would follow. The suggestion was made a decade ago by Lindegren and Bridges (1938) that the specificity of attraction between chromosomes was explicable as an antigen-antibody interaction. The details of the mechanism proposed by these workers might require some change, but the principle involved in accounting for the specificity is certainly worthy of consideration.

Because sperm cells can be obtained in moderate abundance it would be expected that they would offer material second only to the blood for serological studies of the antigenic structure of cells. Reports by Henle et al. (1938, 1940) are indicative of the technic required for such tests. They were able to break mammalian sperm cells by sonic vibrations into heads and tails, and to separate these two parts readily by slow centrifugation. Following this separation, they demonstrated (a) a heat-stable antigenic substance common to both heads and tails, and (b) distinct heat-labile substances of the head and tail, respectively.

That there may be individual differences in mammalian sperm is suggested by the experiments of Landsteiner and Levine (1926), who found that the sperm cells of humans of the appropriate type would absorb specifically and almost completely the immune antibodies (from rabbits) to the A and B characters of human erythrocytes. The A and B substances have been found in nearly every tissue of the body, including testicular tissue, so it would not be surprising if they were present also in sperm cells. If so, they would appear to be more or less direct products of the causative genes. But the finding that blood cells seemingly can "acquire" an antigenic substance from the fluid surrounding them—as will be discussed later—raises the possibility that the sperm cells may simply have acquired these A and B substances from the sem-

inal fluid, which itself contains the specific substances of the blood cells.

For some reason, which one author (Cooper, 1946) has ascribed probably to technical difficulties, many of the attempts in the first part of this century failed to detect in the eggs, embryos, and larvae of amphibians certain antigens which are present in the adults. Much the same picture of conflicting results exists for the various immunological studies of vertebrate embryos. The writer is reminded of one reason given by a co-worker of Landsteiner for the ability of the latter to succeed more often than others in immunological differentiation of biological materials. "You see," said this man, "if an immunization of one animal does not produce results, Landsteiner uses five. If no one of these five provides an answer, he uses ten, and if necessary twenty or thirty." It may be added that in the writer's experience, during nearly two decades, only 2 of more than 300 rabbits injected with the same kind of cells have produced antibodies against each of certain antigens known to be in those cells.

Because of the conflicting results of earlier workers, it is encouraging indeed that two recent studies have demonstrated the presence of antigens or combining groups in embryological development of the chick and the frog, respectively. Cooper (1946) has reported from studies of the common leopard frog (*Rana pipiens* Schuber) that antigens or combining groups closely related to or identical with those of adult serum were present in the eggs, embryos, and larvae. Burke, Sullivan, Peterson, and Weed (1944) have detected antigens of the red blood cells in chick embryos at four to five days of incubation, antigens at least similar to if not identical with the lens antigens of the adult eye at approximately six days, to those of adult kidney at slightly more than eight days, and to those of the adult brain, testis, and ovary at about the eleventh day. That there seemed to be a change in the antigens of the lens with development may be deduced from the reactivity of 90-hour lens antigen with an antiserum to 160-hour lens, but not with antiserum to 300-hour lens; antigen from 120-hour lens was reactive with antiserum to 300-hour lens but not with that to adult lens; while antigen from 160-hour lens was slightly reactive with antiserum against adult lens. Furthermore, when adult lens antiserum was injected into eggs carrying embryos of 134 to 337 hours, various changes were noted in the eye lens and surrounding tissues of 146- to 192-hour embryos, but not in those further advanced. (The authors suggested that the lenses may have hardened in the embryos beyond 192 hours of incubation.) However, if antisera to lens from 160-hour embryos were injected into developing embryos, all died. As stated by these workers, the antiserum may not have been specific to the lens, because of the difficulties in getting this material free of surrounding cells, etc., and therefore may have affected other organs, so as to cause the death of the embryos. From their work it seems clear that unless the proposed differences in the antigenic substances in the embryos and the adults can be attributed to differences in specificities within a small number of immune sera—this being a constant possibility—there can be little doubt but that antigenic changes during development of the chick have been demonstrated.

Burke et al. also observed reactions with the red blood cells of chick embryos at 4–5 days or 100 hours. In fact, until very recently it would have been reasonable to believe that all antigens of the blood cells were formed in the embryo. For example, the A and B characters of human erythrocytes have been found as early as the 37th day in the developing fetus (see reference in Wiener, 1943); and Keeler and Castle (1933) demonstrated that a contrasting pair of antigens in the rabbit (H_1 and H_2) were detectable at the 4 mm. stage.

It is therefore a comparatively new finding that there are some cellular substances which are not detectable in the embryo. Thus, several cellular characters of sheep cells, which are reactive with immune serums, are present at birth (Ycas, 1949). In contrast, a component of the cells of adult sheep, which is recognized by its interaction with a normal antibody, has not been observed on the cells of newborn lambs, but usually has appeared during the second and third week after birth. Although the cells of these lambs did not give any sign of reactivity with the normal antibody to this specific substance, the serum of each of the lambs seemingly contains a substance related to it, if not the same substance. That is, the serum of the newborn lamb inhibits the reaction between this normal antibody and other cells which are known to possess the particular cellular character. It is highly probable that the J antigen of cattle cells, also detected by a normal antibody, behaves similarly.

Another finding from our laboratory is that there is a difference in the time of appearance of various cellular antigens of the chicken. Briles, McGibbon, and Irwin (1948) have demonstrated the antigenic effects of two series of multiple autosomal alleles in chickens. Each of the known members of one series has beeen found shortly after the third day of incubation, or at slightly more than 72 hours. In the other series, one substance was also detected early in the fourth day of incubation, but the other three substances in this series appeared only after the chicks had hatched. These findings in sheep and in chickens emphasize the desirability of studying specific substances during development—whether they be different proteins of the various fluids or formed elements of the cells.

Now that methods are available for the reasonably exact separation of a mixture of proteins, such as the serum proteins, into at least certain of the respective components, it is to be expected that future studies involving developmental aspects will be made, whenever possible, on the particular proteins. For example, it has been recognized for decades that the newborn ruminant receives protective antibodies from the dam by ingestion of the colostrum, and not by the passage of antibodies through the placental wall. But it has been less well known that the blood of the newborn calf, and of the lamb (unpublished data by Cohen and Thompson), is nearly or entirely lacking in γ-globulins—those which contain all or nearly all the antibodies. Following the earlier work on this subject by Howe (1921), Hansen and Phillips (1947) have reported that there is an immediate increase in the γ-globulins of the blood serum of calves following the ingestion of colostrum during the first 24 hours after birth. But if colostrum was fed after the first 24 hours, there was no measurable increase in the γ-globulins. If calves were raised on an adequate diet without access to colostrum, the various fractions of the serum proteins did not approach normal values until the animals were about eight weeks of age. During this period the amount of γ-globulin increased, that of α-globulin decreased. Thus it appears that for a calf the first 24 hours after birth is a critical period for it to obtain gamma globulins from the colostrum, probably because of the greater permeability of the walls of the gastro-intestinal tract during this period.

Response of embryos to immune sera and implants of organ tissue

Some very interesting but quite unexpected results were obtained by Weiss (1947) in experiments designed to determine the effect on the growth of embryonic organs when subjected to antibodies against respective adult organs. The details of these studies are not yet available, but certain findings are noteworthy. In this work, immune guinea-pig sera against either liver, kidney, or pectoral muscle of adult chickens were deposited near the embryo of hen's eggs ranging from 60 hours to eight days of incubation. Preliminary results have been published on only one group of embryos, the tissues of which were fixed on the 20th day of incubation. In these experiments, the chicks injected with muscle antiserum served as controls for the general effects of the antiserum injections, and those treated with antisera to kidney or liver should have revealed any specific effects on the homologous organs.

As one effect of the injections, there appeared to be a general depression in the growth of the treated as compared with that of the untreated embryos since the average weight of the injected embryos was considerably below that of the controls. But in the group of embryos injected with antiserum against extracted liver, the average weight of the liver was about 10 per cent greater than in the untreated, whereas the liver weights in the embryos treated with anti-kidney or anti-muscle sera were less than in the controls. On the other hand, although the average kidney weight in the embryos of the group given anti-kidney serum was greater than the kidney weights of those given either anti-liver or anti-muscle serum, it still was less than that of the untreated embryos.

It might be anticipated, as Weiss stated, that any result of the treatment with antiserums would be one of depression rather than the observed effect of stimulation. Weiss suggested: "If corroborated, it would prove that antibodies to a given organ protein can act as catalysts in the synthesis of more of that particular protein. This would lead us directly to a "template" concept of growth, with molecules of complementary configuration acting reciprocally as moulds for each other's synthesis. In that case, the experimental transfer with double reversal, from chicken to guinea-pig to chick, would actually have been but a complicated version of what happens within the organism itself without

transfer. Each differentiating cell would contain complementary key compounds, each of which would act as a mould for the other."

Further tests were carried out in which small fragments (0.5 mm^3 in size) were taken from livers of 6-day chick embryos and implanted in the area vasculosa of 4-day hosts. The effect on the development of the host embryos was very striking. There was a slight general depression of growth of the treated embryos in comparison with that of the controls, but the livers of the hosts were considerably enlarged, in some cases were three times heavier than the heaviest of the controls. The growth of other organs besides the liver was affected by the transplants of liver tissue, and implants of organs other than liver produced an increase in the size of the liver, although the homologous effect was always strongest.

Weiss suggests (1947) that "either specifically shaped parts of liver cell proteins can act directly as nuclei for further synthesis when entering the appropriate environment, which is another liver cell, or they act merely as models for moulds which then would turn out more of the original product. The latter alternative would bring the experiments with direct liver implantation and those using liver antisera to a common denominator. However, our information is far too sketchy for detailed speculations."

Until more knowledge has accumulated in this intriguing and promising attack on the problems of growth, further comments appear to be without purpose. But it may be well to mention in passing that the immunological findings on organ specificities do not reveal the specificities of different organs to be absolute. Rather does it seem that although each organ may have its antigenic specificity, there is also a more or less common antigenic foundation shared among the organs and probably among all the cells of the body. The preliminary data of Weiss on the effects of the implants were more in agreement with what might be expected if there were no sharp specificity than were those following the use of the antisera to the different organs. Also in his experiments the antisera employed were engendered against adult tissues, whereas the implants were made from embryonic tissue. And since, as stated above, Burke et al. (1944) did not detect in the embryos of chicks prior to the eighth day any substances that would react with antisera to an adult organ, the kidney, there remains a question of the specificity of the effect.

Mention should perhaps be made that a presumed immune response need not always result in a depressing effect. For example, Snell, Cloudman, and Failor (1947) made a preliminary report of results obtained following the injection into inbred mice of frozen-dried (lyophilized) tumor tissue prior to the inoculation of the living tumor. The effects varied according to the strain (a) of the host, (b) of the frozen-dried tumor, and (c) of the living tumor. Inhibition of the growth of the living tissue would ordinarily be expected to follow the injection of the frozen-dried tissue, and this was observed in some instances. On the other hand, at least in one combination, the growth of the living tumor tissue was stimulated by the preceding treatment. A lack of response in some inbred strains to the frozen-dried tissue would not be surprising, for it is well known that animals do not always produce the antibodies expected from immunization, and an inbred strain might be uniform in this respect toward a few antigenic substances. But why a previous contact with an antigenic substance should result in the stimulation of growth of the living tissue is puzzling. Further evidence of what appears to be a stimulatory rather that a depressing effect of immune serum has appeared in several reports which are reviewed by Straus (1946). The principle underlying the experimental work covered in these reports was that the functional capacity of the reticulo-endothelial system of an animal could be improved or kept at optimum by the injection of small doses of a cytotoxic serum. This immune serum was produced in animals by the injection of antigens prepared from organs rich in reticulo-endothelial tissue, as spleen and bone marrow. (The writer is indebted to Dr. W. H. Taliaferro for calling his attention to these papers.)

The preceding examples indicate accomplishments in fields of development, and point the way to various experiments which may be expected to give information on this important subject. As one example, it is now possible to follow with reasonable accuracy the development of various proteins by the electrophoretic pattern of their mobilities. Also, making use of the newer methods of fractionating proteins, it is feasible to undertake experiments in development to trace these different proteins and to compare their specificities. In any

such studies the technics of embryology, immunology, and physical chemistry are mutually complementary.

STUDIES ON PARAMECIA AURELIA

Several reports have appeared within the last few years of the effects of placing representatives of various clones of *Paramecium aurelia* in varying dilutions of antisera to this organism (see Kimball, 1947, for references). According to Bernheimer and Harrison (1940), these animals in dilute antiserum have usually exhibited a gradual decrease in rate of locomotion; they next sink to the bottom, exhibit further dysfunction of cilia, contractile vacuoles and undulating membrane, and finally die. This effect may be essentially duplicated in low dilutions of normal serum, provided the complement of the normal serum is active, whereas in immune serum the presence of complement is not necessary for immobilization of the animals.

One interesting point in these studies is that different workers have observed that some animals, when exposed to an appropriate dilution of antiserum, become "serum-insensitive" or "resistant," in that they regain their power of locomotion and are not immobilized upon further exposure to the antiserum. Kimball (1947) reported that the descendants by vegetative multiplication of these animals remained resistant for periods ranging from one to more than fifteen fissions, but that the resistance was finally lost if the lines were carried long enough. Sonneborn had earlier found (1943) that this resistance was lost, following conjugation with a non-resistant individual. Harrison and Fowler (1945), using the same general approach, obtained several resistant clones, five of which remained insensitive during the test period of 15 to 17 months.

If the so-called resistance of these clones had lasted no longer than 8 to 10 asexual generations, it is conceivable that this phenomenon would come within a strictly immunological explanation. That is, one would need only to propose that the paramecia which were exposed to the antiserum in some way regained the power of locomotion but still were coated with antibody. The antibody already present on the animal might be in sufficient concentration to protect it from a subsequent contact with the antiserum. This antiserum would be diluted one-half its concentration at each fission of the organism, there would be some antibody lost by elution, and within ten fissions or less the antibody on the organism would probably be so dilute that it would no longer act as a film to avoid taking up more antibody. This explanation could conceivably obtain for some of the cases in which the resistance of the paramecia lasted for about 10 fissions.

The actual explanations invoked for this phenomenon were quite different. Harrison and Fowler proposed that their results were due to a selection of naturally occurring mutants, and Kimball postulated that the immune serum induced the changes. Under this latter postulate, the antigens involved in immobilization were assumed to be somewhat comparable to the killer substance in *P. aurelia* in being self-duplicating, as is ascribed to the kappa substance. This explanation requires the assumption that the antigens are definitely interacting in some manner with the antiserum to account for their assumed inactivation or loss.

More recent work with paramecia may provide another possible explanation of these observations, as will be mentioned later. Sonneborn and LeSuer (1948) have presented evidence of the existence of four antigenic strains, A, B, C and D, which arose within race 51, variety 4, of *Paramecium aurelia*. At high dilutions (1:400) of antiserum to the respective strains, the antisera were reactive only with the homologous strain of the animals. However, at lower dilutions, each antiserum was reactive with at least two other strains, and it is probable that reciprocal reactivity would have been observed among all four antisera and strains if many antisera had been tried. These antigenic types appeared to be governed by the cytoplasm, since the antigenic types of the descendants following conjugation between different types depended upon whether cytoplasm was exchanged or not, and upon the amount exchanged.

As might reasonably be expected from the seeming specificity of antigens in each strain, the absorption of an antiserum against one strain by organisms of another strain revealed that the antibodies elicited in immunization were highly specific. That is, absorption of antiserum to type C animals by those of either type A or B removed antibodies only for the absorbing organisms and the absorbed fluid remained reactive with each of the other types. (Sonneborn advises that this finding was observed also with antisera to types A, B, and D.) These findings are in agreement with expectation from the standpoint of immunology.

However, if anti-C serum was absorbed with type C organisms, the antibodies were removed for the absorbing organisms (the homologous type C) but not for types A or B. This result is almost without precedent in immunological experiments, for within the reviewer's knowledge, the absorption of an antiserum by the homologous material has nearly always resulted in a complete exhaustion of the antibodies of that antiserum. The only sorts of exception to this statement known to the writer in bacteriology are the inability (a) of an encapsulated pneumococcus to absorb the antibodies to an R-form, and (b) of flagellated typhoid organisms to absorb antibodies to somatic antigens. In each of these two cases the antibody is against an antigen of the cell which is not at the cell surface.

Since writing this paper, a report by Moody (Moody, P. A. 1948. *Contrib. Lab. Vert. Biol. Univ. Mich.*, 39: 1–16) has been received which states that an antiserum to the red blood cells of a geographical race of *Peromyscus maniculatus* was not completely absorbed by the homologous cells. However, in this instance it seems that the possibility of individual differences in the bloods of mice used in immunization can not be entirely excluded. Also, Dr. Taliaferro has kindly called the attention of the writer to examples in protozoa and helminths of non-absorbability of antibodies by the homologous organisms. Descriptions of these findings and references may be obtained in the following papers: Taliaferro, W. H. 1932. *Amer. J. Hyg.*, 16: 32–84; Campbell, D. H. 1938. *J. Immunol.*, 35: 456 –476.

How can this sitation in paramecia be explained? Definitely, one must admit that antigens similar to or identical with those of types A and B were present in type C cells, to have elicited the antibodies to these two types in immunization with type C animals. (This statement is based on the assumption that the animals used in the production of the respective immune sera were uniformly of one antigenic type, and not mixtures.) The antibodies to these two types (A and B) were absorbable by the respective types but not by those of the homologous type C. Seemingly, then, an additional explanation is needed to that of the "primary" and "secondary" antigens as proposed by Sonneborn and LeSuer (1948). The so-called secondary antigens must be situated somewhere *within* the organism and far enough away from the surface so that they did not combine with their specific antibodies in absorption. Thus the *position* of the secondary antigens, as well as the possible quantitative differences of each type from the primary antigen, appears to be a consideration. This possibility was previously suggested by Sonneborn (1947).

What seems to the reviewer to be a most significant happening is that each antigenic type seemingly was altered following exposure of the organisms to their specific antibody. For example, the undiluted antisera to one type were mixed with sufficient organisms of the other three types to remove antibodies to all the secondary antigens. The four antisera absorbed in this way then became reagents specific, respectively, to each of the four antigenic types. Following subjection of types A, B, and D to their respective specific fluids, a marked change in antigenic types was observed, the frequency of change being increased 200 to 800 times that observed in control fluids. However, it was noted that the organisms of type C changed their antigenic type so frequently *without* being exposed to the specific antibody that this strain originally was considered too unstable for such experiments. Thus it is reasonable to conclude that the organisms of type C spontaneously underwent an antigenic change paralleling those in types A, B, and D, which were markedly increased in frequency by exposure to specific antibodies. It is quite possible that at least many of the individuals which became resistant to immune sera, as mentioned earlier, did so by virtue of an antigenic change, although the resistant individuals of Sonneborn (1943) reverted to the original types at the first fertilization, while those which underwent antigenic change were more or less constant thereafter.

Not only did specific antibodies act in some manner to cause these organisms to change antigenic type, but later investigations (Sonneborn, 1948) have shown that changes in either the temperature or in the amount of available food, or both, will result in such a change. For example, in stock 51, with no exposure to antiserum, at 32°C type A remains uniform, but each of the other types—B, C, D, and a new type E—changed to type A. At 12°C type B does not change, but the other types shift to type B. It was mentioned previously that organisms of type C were quite unstable. In later tests it was found that this type, and in fact all types, remained stable at the standard temperature of 27°C, if each was provided with food sufficient for only one fission per day (Sonneborn, 1948). This finding of the effect of

temperature on antigenic type is probably the explanation of the observations of Kimball (1947), that lines maintained at 14°C became resistant to antiserum, but that the effect was usually not long retained when these lines were grown at 27°–29°c. However, there remains the possibility that the variety used by Kimball may react differently to changes in temperature.

As stated above, the antigens of types A, B, C, and D did not appear to be under direct genic control; at least it was not detectable by the usual genetic tests. Sonneborn (1948) found that these four types of stock 51 were present in another stock of paramecia, No. 29. He also detected in stock 29 a type called F, which was not found in stock 51. By crossing type F of 29 with stock 51, it was noted in the F_2 generation that half the clones either possessed antigen F or were transformable to it, the other half could not be changed to F. The progeny from backcrosses likewise showed a segregation according to genetic expectation. Sonneborn (1948) states that "the capacity to yield clones of type F thus depends on a single chromosome, and presumably a single gene, present in stock 29 and lacking in stock 51. This gene determines not the realization of type F, but only the capacity to realize it under definable conditions. If the other antigens show similar mechanisms, it may be concluded that the genes control the antigenic possibilities, while the cytoplasm controls which of the possibilities is realized and perpetuated."

It is not the purpose of this paper to discuss further the main hypothesis that has already been advanced of the probable role of plasma genes in determining these antigenic types. However, the mere fact that subjection to a specific antibody seemingly produces an increase in the number of these antigenic changes argues that it may be possible to gain some insight as to the mechanism of this phenomenon. For example, the concentration of electrolytes in the dilutions of antiserum may have some bearing on the interaction between antiserum and paramecium. It is well established that the gamma globulins of a serum—in which the antibodies are found—will precipitate in a low concentration of electrolytes, i.e., in a high dilution of antiserum in water, rather than in a salt solution. That is, if water has been the diluent in these various studies, is it not possible that a precipitation of gamma globulin is in some manner connected with this phenomenon? If so, the shift in type could conceivably be a response to a physical state, and not to the antibodies as such.

It would be most interesting and instructive to know more in detail of the immobilizing effect of antisera on the organism, and thereby perhaps to be able to locate in each antigenic type the site of the primary antigen. If this effect is mainly on the cilia, what inferences may be drawn regarding the change of antigenic types? Can the secondary antigen be far enough below the surface of the cilia to escape during absorption the long range forces between antigen and antibody postulated by Rothen (1947), if these forces are a reality?

There are other biological phenomena, particularly in parasitology, which appear to parallel these intriguing changes in antigenic type in *P. aurelia*. For references to these, see the review by Harrison (1947). For example, in relapsing fever, the spirochaetes recovered from some patients are not of the same antigenic types after respective successive relapses. And pathogenic trypanosomes in the guinea-pig often show an alteration in antigenic type at the time that the majority are acted upon by the antibodies of the host, so that a few trypanosomes survive. These alter antigenic type so that they are not acted upon by the circulating antibody, they multiply, the host produces antibodies against the new type, and all but a few are eventually destroyed; these in turn alter antigenic type and multiply, engender antibodies, and so on. This process may be repeated several times before the host dies.

Currently there is much interest in many laboratories regarding the ability of certain viruses to agglutinate the red blood cells of various animal species. Different specificities are exhibited by some viruses. For example, after influenza virus B has acted on chicken red blood cells, the same cells will no longer react on a second exposure to this virus, but will react to Newcastle virus. One explanation which has been offered of this phenomenon is that some specific part—possibly a carbohydrate—of the red blood cell reacts with a particular virus, and is neutralized thereby, or maybe sloughed off (eluted). Another specific part is likewise reactive with a second virus, and it in turn can be neutralized or eluted.

In *P. aurelia*, if upon treatment the antibody to an antigenic type is bound to the cell, it should be possible to demonstrate its presence. On the other hand, if the specific antigen is sloughed off, it is conceivable that its presence might likewise

be demonstrated in the diluent. But if the antigenic change follows a sloughing off of an antigen, rather than a shift in the relative position of the antigens, either some of the antigen is left in or on the cell, or it can be reproduced by some particle within the cell, to account for a reversal to the original type. The experiments of Beale (1948) are suggestive that the antibody is attached to the surface of the paramecia, for less antibody was absorbed in a second exposure to the antiserum than at the first contact. Information of this kind should aid considerably in an understanding of the processes involved in the change of antigenic types, and may be anticipated in the future.

STUDIES IN SPECIES RELATIONSHIPS

Let us turn now to antigenic characters of the red blood cells which appear to be entirely under genic control. Previous papers have described the isolation in unit form of the cellular characters which distinguish each of two species of doves, Pearlneck (*Streptopelia chinensis*) and Senegal (*St. senegalensis*), from Ring dove (*St. risoria*). In both Pearlneck and Senegal the segregation of the various substances in backcrosses to Ring dove has simulated that expected if one or more genes on each of a given number of chromosomes—assuming that no linkage exists—produced these cellular characters. Thus in Pearlneck there are at least nine chromosomes with one or more genes producing, respectively, the specific antigens of Pearlneck, and in Senegal there is a minimum of ten such chromosomes. Since it has been found that the major part of the antigenic substances *common* to Pearlneck and Ring dove are shared also with Senegal, and those *common* to Senegal and Ring dove are shared with Pearlneck, the differences between Pearlneck and Senegal fall primarily within the substances of each species not shared with Ring dove, i.e., those specific to each species. At least the majority of these, if not all, have been isolated as single entities. Hence a comparison of the so-called unit antigens of Pearlneck with those of Senegal should give an accurate picture of the relationships of these substances, from which may be deduced the similarities and dissimilarities of the causative genes.

The technical details of these comparisons and references to preceding papers are given elsewhere (Irwin, 1947), and the relationships deduced as a result of these comparisons are shown diagrammatically in Fig. 1. In this diagram the antigenic constituents of Pearlneck cells are shown as of two kinds: (a) those *common* between Pearlneck and Ring dove, and (b) those *specific* to Pearlneck. The components specific to Pearlneck have been separated in backcrosses to Ring dove into the substances named d-1, d-2, d-3, d-4, d-5, d-6, d-7, d-11 and d-12. Likewise, the antigens specific to Senegal as contrasted with Ring dove have been divided into s-1, s-2, s-3, s-6, s-7, s-8, s-9, s-10, s-11 and s-12.

In the diagram complete cross-hatching of two blocks representing two cellular characters, e.g., d-3 and s-11, connected by a line indicates that these were immunologically indistinguishable and therefore presumably identical. This implies that the respective chromosomes in these two species carry seemingly identical genes—at least those effecting antigens of the blood cells—and are therefore homologous. The antigenic characters which are probably identical in Pearlneck and Senegal are d-3 and s-11, d-4 and s-6, d-5 and s-1, and possibly d-12 and s-9. It may logically be inferred that these are produced by genes on four homologous chromosomes of these two species, and that these four chromosomes in each species carry one or more genes with effects differentiating each from Ring dove. (It is of course possible that tests with different antisera will show these supposed homologous cellular substances to have different specificities, and any such differences would also be reflected in the causative genes.)

There are several examples given in the diagram of antigens which are similar but not identical in these two species, as d-2 and s-7, d-6 with some substance of Senegal which may be s-3, and d-11 with s-8. The major part of the Pearlneck substance d-7 is also shared with a substance or substances in Senegal, but this cellular character of Senegal has not yet been detected. As has been stated before, this kind of relationship is subject to either of two explanations, or to a combination of both. (1) If such a substance in each species is produced by the action of a single gene, the relationship between the products of these genes in the respective species would be that of antigenic similarity, not identity. Presumably this same relationship of similarity but not identity would likewise obtain between the respective causative genes. (2) But if two or more genes on a single chromosome in each species acted together to effect each such substance, as d-2 in Pearlneck and s-7 in Senegal, one or more genes could effect parts

common to these antigenic characters, and one or more could produce the *specific* parts. The related chromosomes would then carry some homologous genes, and some not homologous.

The antigenic character d-1 of Pearlneck has behaved as a single substance in the backcross generations involving Pearlneck and Ring dove. But, as is shown in the diagram, it is definitely related to both s-2 and s-12 substances of Senegal. However, the s-12 substance of Senegal, in addition to being related to the d-1 character of Pearlneck, carries properties specific to Senegal. Unless the

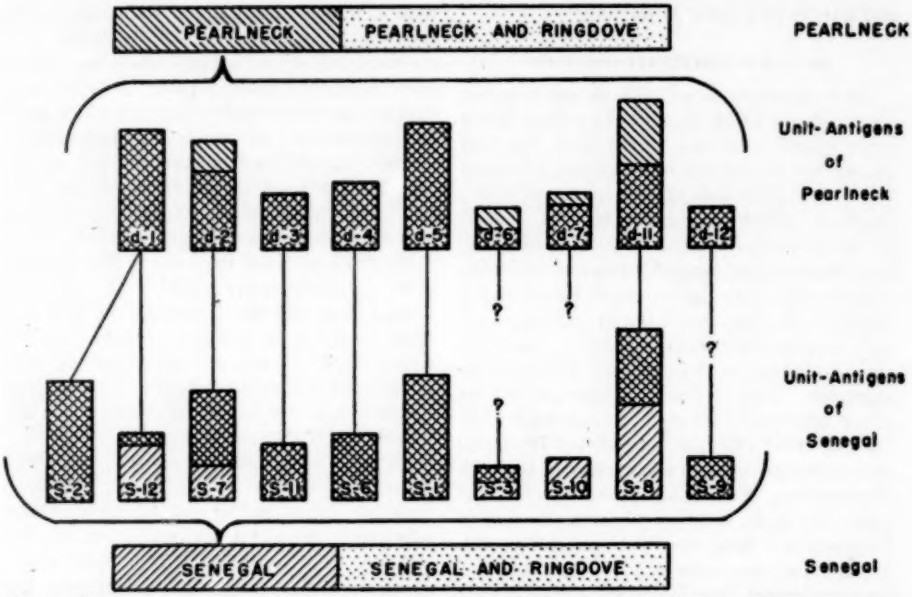

FIG. 1. DIAGRAM OF THE RELATIONSHIP OF THE ANTIGENS OF PEARLNECK AND SENEGAL, RESPECTIVELY, IN CONTRAST TO RINGDOVE

chromosomes originally derived from Senegal, which effect s-2 and s-12, respectively, are the result of a crossover in the species hybrid, it appears that a gene or genes on two independent chromosomes in Senegal produce antigenic substances very closely related to, if not identical with, those produced by one or more genes on but one chromosome in Pearlneck. This finding strongly indicates that a chromosome aberration of some kind occurred during the evolution of these two species from a common ancestor.

The best estimate of the number of chromosomes in pigeons and doves is that provided by Painter and Cole (1943), who found 30-odd pairs in the pigeon (*Columba livia*) and in the Ring dove. Assuming that the same number exists in both Pearlneck and Senegal, approximately one-third of these (10 of 30-odd) in the two species carry a gene or genes which produce antigenic effects on the red blood cells which distinguish each species from Ring dove. However, of those which differentiate Pearlneck from Ring dove, there are four which have homologues in Senegal, i.e., those effecting d-3, d-4, d-5, and probably d-12. A fifth, that with genes producing d-1, appears to have homologous genes on one chromosome of Senegal—with genes effecting s-2—and also some homologous to one or more genes on another chromosome whose genes act together to produce s-12. In Senegal, there are five chromosomes with homologues in Pearlneck—those with genes producing s-1, s-2, s-6, s-11, and probably s-9. There are also in Pearlneck three or four chromosomes carrying genes effecting only similar antigenic substances—d-2, d-6, probably d-7, and d-11. In Senegal there are also four such chromosomes—with genes effecting the antigenic substances s-3, s-7, s-8, and s-12. Another chromosome, with a

gene or genes responsible for the s-10 substance, is seemingly entirely unrelated to any in Pearlneck. Thus Pearlneck differs from Senegal by virtue of genes for cellular antigens on four chromosomes, and Senegal differs from Pearlneck in the effects of genes on five chromosomes.

It is interesting that these species can be differentiated sharply by antigenic substances in either cells or serum, whereas certain measurable morphological characteristics—such as over-all length and extent, length of wing, beak, tarsus, middle toe, and tail feathers—do not permit a differentiation except on statistical bases. Analyses of measurements of these characteristics, taken by the late Professor J. L. Cole and as yet unpublished, indicate that each behaves in the species crosses of Pearlneck and Ring dove, Senegal and Ring dove, Pearlneck and Senegal, and in the several backcross generations as would be expected if each were produced by multiple genes, and if their effects were cumulative. Therefore the odds against finding a correlation between any of these morphological characters and the antigenic characters specific to one species appear to be considerable.

STUDIES IN GENE ACTION

The observations that genes on many chromosomes of a species produce antigens of the red blood cells, specific to the species, provided a basis for the assumption that many such effects are present and make for individuality within a species. The fundamental requirement for such studies is that each substance be recognized singly, as are the A and B, and the M and N antigens of human red blood cells. A considerable amount of unpublished work with chickens has provided information that the erythrocytes of that species possess numberous antigenic substances, as expected. Comparable studies in cattle have progressed more rapidly, largely because of fewer technical difficulties in respect to the immune sera. At present, over 40 antigenic substances are detectable in cattle cells, by virtue of the reaction (i.e., lysis) of each substance with a specific reagent or test-fluid. These antigenic characters have been labelled alphabetically, usually in the order of their discovery. For example, the first substance identified was called A, the next B, and so on through Z, the next one then being named A′, etc.

As stated above, each of these antigenic substances of cattle cells is recognized by virtue of its reaction with a specific reagent. There are a few exceptions to this general rule, involving what are termed the subtypes of a few cellular characters, but these need only be mentioned at this time. Further, each antigenic character has behaved in inheritance as if produced by a single gene which was either dominant over its absence—as are A and B to O in human cells—or the effect, if any, of its allele has not yet been detected. Obviously, it has not been possible to make critical matings in cattle for direct tests of the manner of inheritance of any of these substances, and the basis for the above statement rests upon a gene frequency analysis of each antigenic character, using only animals of known parentage.

Although each of these 40-odd cellular characters has behaved as if produced by a single gene, it was noted a few years ago that there were definite associations among certain of them. For example, Stormont first observed that the character called K never appeared alone, but was demonstrable only when both B and G were also present. That is, each animal which possessed K in its cells actually had the so-called "antigenic complex" BGK. The substance B, however, did appear as a single entity, as also did G. A fourth component, I, has never been observed in a complex in association with K—therefore of course not with BGK—but has been found in association with either B or G.

Counting the so-called subtypes of certain of the cellular substances, there are 21 of the 40-odd characters known at present which are associated in a single series in various combinations or antigenic complexes. Each of these involves from two to eight of the 21 substances. Seven of the 21 have appeared singly, among them the B and G components; the others have been found only in various combinations. Genetically these complexes may be accounted for, either by assuming (a) closely linked genes, each of which effects a respective part of the antigenic complex, or (b) a series of multiple alleles, each member of which produces a cellular character, or antigenic complex. The strongest evidence against the hypothesis of linked genes is that no separation of the antigenic parts of any complex possessed by an individual has ever been observed in the cells of its offspring. Admittedly, the number of offspring from an individual in cattle is relatively small, but reasonably adequate numbers are obtained by grouping the offspring from various individuals. For example, each of more than a thousand animals

whose cells carried the K substance has also contained the B and G components. If linkage of the causative genes is involved, it must be very close indeed. Further, certain of these antigenic components behave exactly as would be expected if they were produced by allelic genes, but these results would not be understandable if the causative genes were closely linked, yet might cross over. Thus, no animal unless a twin has ever possessed all three of the cellular characters K, I, and T. Any two of these—as IK, KT, and IT—may be found in one individual, with complete separation in the offspring as would be expected if the causative genes were alleles. Hence, although one cannot dismiss completely the possibility of gene linkage in the production of these antigenic complexes, the present data make it much more reasonable to assume that each antigenic complex is controlled by a single gene in an allelic series than by linked genes. (Actually there are two such probable allelic series recognized, although only one will be considered here.) The details of the data in support of this argument are presented elsewhere (Stormont, in press; Stormont, Owen, and Irwin, 1948, and in press). This proposal also has confirmatory evidence in another species, since Briles (1948) has demonstrated in the cells of chickens a parallel situation of antigenic complexes, and they are produced by genes seemingly in an allelic series.

We come now to the main reason for considering these antigenic complexes, since it seems reasonably certain that some light may be given by these studies on the action of genes. Let us consider a few relatively simple cases. It was stated earlier that the cellular characters B and G may appear singly or, if both are present, in combination with K. Further, the combination BG may occur without K and be of two kinds. One of these is the heterozygote, and the offspring of such animals will be of two classes: half with B and half with G. In the second class of BG individuals, in appropriate matings either all the offspring will possess BG, or half the offspring will have BG, the other half will not possess either of these characters. That is, in this second kind of individual the distribution in the offspring is that expected if a single gene produced both B and G. Similarly, another gene in the series is presumed to produce the complex BGK.

At first thought it would seem reasonable to conclude that this is an excellent example of a multiple allelic series of genes in which there are some genes, as that effecting the complex BG, which can produce a compound substance whose two components are produced by two distinct alleles, those for B and G, respectively. In favor of such a conclusion it should be repeated that the presence of any single character, or part of a complex, is recognized only by the reaction of the cells with a specific reagent. Thus, the presence of the B antigen alone, or of the B component in any combination as BG, BGK, and others, is detected by virtue of the lysis of certain red blood cells with a specific reagent. But we do not yet have definite evidence whether the reactive substance called B is the same in the various complexes as it is in cells in which the antigen B occurs alone. If the B substance, to use but one example, is the same no matter in what antigenic complex it is found as it is when present in cells as a single character, there could be little question that these antigenic complexes would be excellent examples of pleiotropic effects of genes. And if the B substance is not the same among the numberous complexes as when present alone, it still reamins to be decided whether the antigenic complexes may be examples of pleiotropic effects of genes.

Thus, the question largely revolves around the antigenic makeup of such cellular characters as B or G, in contrast with the participation of either or both of these in an antigenic complex, as in BGK. It is possible, in the first place, that the gene effecting BGK may produce both B and G as so-called reacting or determinant groups and these groups (B and G) might conceivably be less in quantity than if either appeared singly. Or it might be, secondly that the fact that the reagent for B is reactive with the antigenic complex BGK is due to the presence within this complex of a substance similar to but not identical with the B antigen when this occurs alone. Or, thirdly, the various antigenic complexes may resemble each other sufficiently to account for some cross-reactivity, but without having identical reacting groups. Since these antigens are at present recognized only as a result of their interaction with specific antibodies, the most promising experimental approach for an attempt to cast light on this problem appears to be a comparison of the specificities of antibodies elicited to the antigens which appear both singly and in complexes. Such studies are already under way.

TWINS IN CATTLE

Despite the open question of the nature of gene action in effecting the antigenic complexes of cattle cells, each of the 40-odd cellular characters—either by itself or in a complex—has been found in the blood cells of an individual only if one or both parents also possessed it. Furthermore, all the red blood cells of an individual would ordinarily be expected to have the same antigenic constitution. It was distinctly surprising, then, to find that the majority of twins in cattle have two kinds of blood cells—(a) those belonging to the individual itself, and (b) those corresponding to the blood of the co-twin.

It had been noted in scattered tests on a small number of twins that identity of blood types was somewhat more frequent than was expected of supposed non-identical twins, and that identity of blood types in twins did not always mean even similarity of appearance. The assumption had been held that identical twins in cattle, as in man, would have identical blood types and that non-identical twins would be no more alike than sibs. But upon testing the bloods of twins from a case of superfecundation in cattle in which a genetic differentiation of the co-twins was possible, it was found by Owen (1945) that the twins had identical blood types. (Identity of blood types in quintuplets (Owen, Davis, and Morgan, 1946) has also been noted, as well as in other multiple births.) Furthermore, the cells of each co-twin possessed two antigens the genes for which could not have come from its own sire or from the dam. Further tests on many twins have shown that approximately 90 per cent of the pairs have identical blood types. The most rational explanation of this general finding is that advanced by Owen (1945), namely, that the union of the circulatory system of bovine twins, as demonstrated by Lillie (1916), provides a mechanism for the migration of the so-called primordial blood cells and their establishment in the hematopoietic tissues of the co-twin. From that time on they produce blood cells and both twins, therefore, have a mosaic of blood cells. Granting that this migration and establishment of the primordial blood cells may occur reciprocally in twins in cattle, the question seems legitimate as to whether this phenomenon is limited to primordial blood cells. Would it not be expected that the so-called primordial cells (of any tissue) which may migrate could become established in a co-twin?

One exception has been noted to the general rule that most twins in cattle have identical blood types. Some irregularities involving the cellular character J have been noted in the reactions of the cells of twins. This antigen is recognized by its reaction with a normally occurring antibody and behaves as if produced by a single gene. As Stormont (1948) has reported at these meetings, this cellular character J was present in all the cells of one twin possessing a definite admixture of blood, but was not demonstrable at all in the cells of the other twin. Since this J substance is poorly expressed, if at all, on the cells of a young calf, but is present in the plasma, it was reasoned that this substance was supplied to the blood cells from outside, rather than by the hematopoietic tissues. Evidence to substantiate this reasoning was provided when large quantities of blood carrying appropriate markers, but without the J substance, were transfused to animals possessing J in both cells and plasma. The transfused cells gradually "acquired" the J substance. This acquisition of the J character by non-J cells was also accomplished in vitro by mixing non-J cells with plasma containing the J substance. Hence the non-J cells either affixed to themselves the J substances from the plasma, or the plasma containing J effected some change in the non-J cells so that this antigen became reactive. The gene for this J substance seemingly does not produce its effect in the hematopoietic tissues, but produced the J substance so that it is liberated into the plasma and is there taken up by the blood cells as an acquired character. The immediate problems to be investigated concerning this substance would seem to be its place of origin, distribution in the animal body, and chemical nature.

CONCLUSION

The preceding examples of findings in a few divisions of biology by immunological technics are illustrative of what may be accomplished thereby. Another field to which immunology has contributed is that phase of endocrinology which deals with "anti-hormones." Also, there are two other phases of fundamental biology in which the technics of immunology have provided significant results. These have been considered briefly in a previous paper (Irwin, 1947) and will only be mentioned here. One of these deals with the possibility of the induction of genic changes by antibodies. The other is the production of hemolytic disease in the newborn by antibodies from the

mother. In order that the latter condition may be studied experimentally, attempts to induce it have been made principally in certain mammals. However, the finding by Briles (1948) that hemolytic disease in the chick could be induced against certain cellular antigens as a result of the transmission of antibodies to the chick, primarily if not entirely through the yolk, suggests strongly that hemolytic disease may be induced in any animal species in which the young may receive antibodies from the mother, whether by passage through the placenta or otherwise.

If one considers that much of our knowledge of the specificities of proteins, and of simpler substances linked to proteins, whether within or between species, rests upon findings made by the procedures of immunology, it may reasonably be expected that this tool will continue to be indispensable in further studies of substances of living organisms which are not otherwise detectable. Eventually, of course, any such investigations should include the technics of chemistry, so that many of these problems in biology should entail a correlation of the three fields—biology, immunology, and chemistry. To paraphrase the old adage that "two heads are better than one," one may state that a combination of two or more scientific disciplines brought to bear on a biological problem may certainly be expected to provide results not possible by the use of any one of these alone.

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THE CONCEPT OF HOMOLOGY IN THE LIGHT OF THE COMPARATIVE ANATOMY OF VERTEBRATES

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Introduction

IT IS now more than a hundred years since Richard Owen first defined the term homology (1843). The definition of Owen is extremely short and simple: "Homologue. The same organ in different animals under every variety of form and function." Owen made an epochal step in the progess of the zoological sciences, in spite of the fact that this was not the introduction of a new idea. His views on homology had nothing to do with the evolution of animals, but were exactly like those of his predecessors. Like his contemporaries, Owen was looking for a common plan in the structure of vertebrates, but he did not try to find a cause for its existence. Lubosch (1931) points out rightly the influence of Platonism on this scientist. The community of plan in various vertebrates was for Owen no more real than a Platonic idea. Accordingly, the term homology, at the time when it was formed and first defined, did not imply anything more than a purely formal relation, not pointing to any evolutionary or developmental suggestions.

This fact is not always remembered by present-day biologists. That is why the concept of homology has always caused such extensive discussion, and has even led to misunderstanding. Boyden (1943) is of the opinion that all we need is to "adopt Owen's meanings without further delay" (p. 240). We must, however, realize that our views on the living world are now radically different from those held a hundred years ago, and that a simple return to Owen's definition is impracticable, because it is too loose. The words "the same organ in different animals" can be, and are currently differently understood by different writers; and therefore if the ideas of Owen are to be followed, namely, if "the terms are the tools of the teacher, and only an inferior hand persists in toiling with a clumsy instrument when a better one lies within his reach," then we must examine the term in its historical evolution and its current use, especially in the comparative anatomy of vertebrates, in the hope that this will clarify the concept and put an end to our misunderstandings.

The problem of homology arose in its present state after the work of Darwin had introduced the idea of evolution as the basis of biological thought. This fact is plainly shown in the alteration in meaning of the term which is apparent in the works of Carl Gegenbaur. In the first edition of his treatise on comparative anatomy, which appeared in 1859, Gegenbaur defined the term homology quite conservatively, stating that homologies are only found in such organisms as belong to the same structural type. "Homologien werden ... nur bei Organismen bestehen, welche Tieren von einen gleichen Typus angehören."

In the year 1866, however, Haeckel introduced the Darwinian theory of evolution into comparative anatomy and came to the conclusion that homology has a material background, i.e., that it points to the common ancestry of animals having homologous organs. The views of Haeckel deeply influenced the significance of the term homology. According to him, we can define the organs of different animals as homologous when the presence of those organs is due to the common ancestry of those forms.

In the second edition of his famous book, which appeared in 1870, Gegenbaur followed Haeckel and emphasized that homology is a relation which exists between two organs which have the same descent and accordingly can be traced to the same rudiment: "Homologie ist das Verhältniss zwischen zwei Organen, die gleiche Abstammung besitzen, somit aus der gleichen Anlage hervorgegangen sind." Simultaneously with Gegenbaur a very similar definition of homology was put forward by Lankester (1870).

In the second half of the nineteenth century the

evolutionary significance of homology received almost general approval, in spite of the fact that at once some difficulties arose in its application. The difficulties had their origin in the circumstance that the old classical concept was rather vague and therefore more elastic, whereas the evolutionary concept was definite and therefore more difficult to apply.

The difficulties in the application of the evolutionary definition of homology may be grouped under three heads. In the first place we must consider the facts regarding the development of homologous organs from different metameres. Secondly, we shall discuss the origin of homologous organs from different germ layers. Finally, the phenomena of parallel evolution must be taken into account.

THE DEVELOPMENT OF HOMOLOGOUS ORGANS FROM DIFFERENT METAMERES

We are forced to look on the pectoral and pelvic girdles of all vertebrates as homologous structures. Their position in the body is, however, very variable, according to the general proportions of the animal. So, e.g., in amphibians the sacral vertebra is ninth in *Rana*, whereas it is the twentieth in *Cryptobranchus*. If we adhere strictly to the definition of homology given by Gegenbaur, which demands that the homologous parts must be derived from the same rudiment in the embryo, then we may be drawn to the conclusion that the sacral vertebrae in *Rana* and *Cryptobranchus* are not homologous.

Such a point of view, which can seem paradoxical, was indeed considered plausible. The first anatomist to draw the ultimate consequences from the situation was Fürbringer (1876). He observed, namely, that in various vertebrates the muscles of the pectoral girdle develop from different metameres, which is evident even in the adult form, from the distribution of nerves. Fürbringer concluded that those muscles are consequently not homologous, or are only partially homologous.

Instances of similar organs arising from different segments of the body, even in closely related forms, are very numerous. In the Myxinidae the last gill cleft on the left side of the body is modified, as it has no gills, and forms the ductus pharyngo-cutaneus. It is, however, the seventh cleft in *Myxine*, the ninth in *Bdellostoma okinoseanum*, and the twelfth or thirteenth in *Bdellostoma stouti* (Rauther, 1937). We are, nevertheless, forced to the conclusion that in all these species it is a homologous structure. It is well known that in various oligochaetes the sexual glands develop from different metameres; we must see them however, as homologous. A somewhat similar problem caused Bateson (1892) to attack the contemporary concept of homology. *Ateles marginatus* usually has three premolar teeth. Bateson found, however, an animal with four premolars. All the teeth were quite normally developed and not a one showed any sign of later development.

There were several attempts to develop the understanding of such facts. Baur (1897) and Ihering (1878) put forward the assumption that during evolution the number of metameres could be increased or decreased by intercalation of additional segments or by the loss of these. Welcker (1878) suggested that during ontogenesis the same sector can be divided into various numbers of metameres, so eight longer segments in one animal may correspond to ten shorter in another. The same idea was developed by Bateson (1892). Finally, Rosenberg (1899) strongly opposed the above views and supported the opinion that, e.g., the sacral vertebra of various vertebrates cannot be considered as homologous organs at all.

DEVELOPMENT OF HOMOLOGOUS ORGANS FROM DIFFERENT GERM-LAYERS

The second group of problems arises from the fact that obviously homologous structures can be developed in different animals, or even in the same species, from different germ layers. The present status of this question is extensively discussed by de Beer (1947).

Thus Landacre (1907) found that the taste buds can develop in *Ameiurus* quite in the same manner both in the ectoderm and in the entoderm. Adams (1924) pointed out that in the Caudata the enamel organs of teeth based on maxillary and other bones are formed from the ectoderm, whereas the enamel organs of teeth situated on the palatine and splenial bones arise in the entoderm. The findings of Adams were recently reexamined by de Beer and the results confirmed.

Kastschenko (1888), Vogt (1929), Harrison (1938), de Beer (1947), and others proved that, in the region of the head, mesenchyme develops from the material of the neural crest, and thus from the ectoderm, and that in further course this mesen-

chyme gives origin to the visceral cartilages and also to odontoblasts. Hörstadius and Sellman (1946) found that it is the presence of the entodermal epithelium of the pharynx which induces the formation of cartilage from the mesenchyme.

The thymus can develop both from the ectoderm and from the entoderm. So, e.g., dogs and cattle have entodermal thymuses, many species have mixed thymuses that arise from both ectodermal and entodermal rudiments (pig, mouse, guinea-pig), and finally there are species which have thymuses built solely from ectoderm (European mole, *Phascolarctos*, *Phascolomys*) (Hammar, 1936).

Taking these and more examples as his basis, de Beer (1947) has come to the conclusion that the ontogenetic development of an organ is not decisive for the study of homologies and quoted with approval the words of Wilson (1896): "Comparative anatomy, not comparative embryology, is the primary standard for the study of homologies and hence of genealogic descent."

PARALLEL EVOLUTION AND HOMOLOGY

We must now consider the complications which arise from the assumption of the existence of parallel evolution. As stressed by Huxley (1945), if in several groups of a species parallel mutations occur simultaneously, or if these occur in related species, and if these mutations are fixed by natural selection, the new characters which arise in such a way ought to be called homologous, in spite of the fact that they were not present in the common ancestor.

Huxley has developed a classification of parallel evolution. In the first category he groups those characters which have arisen as a consequence of a mutation which changed the same gene in the same way. In the second category, he includes those characters which are similar in spite of the fact that they do not owe their origin to completely identical mutations. This category is further divided into two smaller groups. In the first are such similarities as are caused by different mutations, but which during ontogeny develop in the same way; and in the second are grouped those similarities of structure which are caused by different factors both in phylogeny and in ontogeny. Only these last-mentioned could be called non-homologous, according to Huxley's views.

The existence of parallel evolution in the past of many groups of animals has been demonstrated a number of times by paleontologists. Osborn (1926) wrote that identical horns have developed on identical places of the skull in four distinct groups of tithanotheres. The same authority also demonstrated the presence of parallel evolution in the history of the elephants (1936). Le Gros Clark (1934) has claimed that parallel evolution has characterized the development of the primates.

A well known example of a mutation which affects very distantly related vertebrates is the development of a bulldog face, which appears in many fishes and mammals (Schäperclaus, 1941; Stockard, 1941). Still more common is the lack of black pigment, causing albinism. Both characters can be regarded as homologous, although they are by no means the reflection of features present in common ancestors.

Parallel evolution was demonstrated several times in the Amphibia by Noble (1931). Thus, e.g., digital pads arose independently in several unrelated families of the Salientia (Bufonidae, Dendrobatidae, Hylidae, Ranidae, Polypedatidae, Brevicipitidae) in some genera, while other genera do not show this arboreal adaptation. Even among the representatives of a single genus it happens that some species have the discs and others do not (Mikulski, 1929). Similarly, in several families the true teeth were lost and "pseudoteeth" appeared. Noble closed his discussion with the words: "In Amphibia many characters of dentition, pupil form, pectoral girdle, tongue form, digital scutes, digital loss etc. have reappeared in groups not closely related. It would seem that the various families of Amphibia had only a limited repertoire of germinal changes." In these words of Noble we can see it suggested that the enumerated similarities arose as the result of identical or similar mutations, and if this is true, then, according to Huxley's views, they ought to be called homologous.

An example of parallel evolution can be seen in the development of immobile, transparent eyelids in several reptiles. As is well-known, such a structure characterizes all snakes and is further found in the majority of the Gekkonidae and in a few representatives of other families such as *Rhinophis* (Uropeltidae), *Ablepharus* and *Chalcides* (Scincidae), *Ophiops* (Lacertidae), etc. Especially interesting features are shown by the genus *Eremias* (Lacertidae) in which the development of transparency of the lids varies from species to species (Franz, 1934).

The transparency of eyelids consists in the presence of epidermis which does not become

opaque even when dried. If we consider that only reptiles have succeeded in producing such tissue, and that in all reptiles the origin of transparent epidermis is closely similar, as the closed lids always become transparent and the corneal epithelium never becomes dry without the loss of transparency (Neher, 1935; Bellairs and Boyd, 1947), then inevitably the suggestion arises that the development of transparent lids was caused in all the widely differing groups of reptiles by identical ontogenetic and phylogenetic processes, and that the structure in all representatives ought to be called homologous, even though the remote common ancestors most probably had normally developed lids. We must therefore conclude with Huxley (1945) that "the homology, though perfectly real, no longer implies descent from a common ancestor, showing the common feature."

HOMOLOGY AND EXPERIMENTAL EMBRYOLOGY

Seeing the difficulties caused by the evolutionary definition of homology, some students attracted by experimental embryology have tried to form a new definition based on the achievements of that science. At first glance it seemed easy to find a plausible definition of the term, e.g., homologous characters are those characters which are formed by the action of identical or similar mechanisms during ontogenesis. Moment (1945) wrote: "Most of the similarities or homologies . . . are all similar because due to similar developmental forces." Such a line of thought at once puts aside the majority of the difficulties enumerated. So, for instance, it is highly probable that the sacral vertebrae are characteristically molded in all vertebrates by the influence of the adjacent parts of the pelvic girdle. The taste buds in *Ameiurus* arise as a result of the vicinity of nerve endings (Olmsted, 1920). The enamel organs of the teeth develop probably under exactly similar influences, whether entodermal or ectodermal. The results of parallel evolution are only then called homologous when they develop during ontogenesis in the same way, and so forth.

In spite of such facts, a definition like that given above cannot be regarded as acceptable, for even if old difficulties are avoided by its use, new ones appear which are probably even more serious. Usually during ontogenesis an organ is formed from a rudiment not under the influence of one agent alone, but rather by synergic action of several inductors. The indispensability of various inductors can vary from species to species, even in forms closely related. So, e.g., in some amphibians amputation of the optic cup results in the lack of development of a lens, whereas in other closely related animals the lens will develop even in the absence of the optic cup (Weiss, 1939). We can also suppose that, for instance, the development of the mesoderm in various groups of vertebrates, which takes such a different morphological course, is also ruled by different developmental forces. We are surely right in regarding the nasal ducts of all tetrapods as homologous. In frogs and salamanders, however, these structures arise in the course of development from rudiments in the form of blind pits which subsequently deepen and finally open into the mouth cavity, but in all other tetrapods, including even coecilians, the first rudiments of the nasal ducts have the form of grooves, which are later on closed by strips of tissue that arise from the median walls of these grooves (Peter, 1922).

Similar considerations led Spemann (1915) to the conclusion that it is impossible to build a definition of homology based on the achievements of experimental embryology and not controversial to well-founded convictions of the majority of biologists. Nor did Moment (1945), although emphasizing the value of developmental mechanics for the study of homologies, claim that the considerations of developmental forces would clear up all problems of homology.

DISCUSSION

After examining the present status of the concept of homology, one arrives at disquieting results. A basic term of one of the most important zoological sciences, that of comparative anatomy, cannot be exactly defined. Nearly every anatomist speaks of homologous organs and characters, but most often he does not try to say what homology is, or if he gives a definition of that term, then his definition is contradictory to the majority of definitions put forward by other students of the problem.

Moreover, as rightly underscored by Boyden (1943), it is not difficult to find in current zoological literature a plain misunderstanding of the concept of homology and even sometimes a confusion between homology and analogy. So it is comprehensible that Boyden's article caused quite an extensive discussion (Hubbs, 1944; Moment, 1945; Kälin, 1946; Haas and Simpson, 1946) in

which several points of view on the question were formulated.

As has been said already, Boyden is of the opinion that all difficulties can be put aside by returning to the old definition of Owen. The examples described earlier should demonstrate, however, that neither Owen's nor Gegenbaur's definitions can solve the present problems. Similarly, the recent definition of Boyden (1947): "*Homologous:* essentially similar in the structure and embryonic development and in the relative position and connection of corresponding parts of the bodies of organisms" will not make plain the difficulties enumerated, which arise, e.g., from the development of homologous organs from different metameres. The impossibility of returning to Owen's concept was also stated by Hubbs, by Moment, and by Haas and Simpson. We ought, however, to remember that even more radical views than Boyden were advocated several times. As the best known representative of this point of view we can quote Naef, whose long series of papers was summarized in 1931. According to Naef, the term homology has value only when considered as purely abstract. The definition of homology given by Naef further develops the line of thought of Owen and his contemporaries: "Homologie ist die formelle Beziehung zwischen bestimmten Teilen der Gesamterscheinung verschiedener planänlicher Organismen (oder verschiedener planänlicher Ausschnitte der Gesamterscheinung eines Einzelnen) welche sich daraus ergibt, dass diese Teile den anderen des zugehörigen Ganzen (oder Ausschnittes) in naturgegebenen Zusammenhang in überstimmender Weise zugeordnet sind und daher im gemeinsamen Plan durch je einen einzigen Bestandteil dargestellt werden können." Any attempt to relate homology to mode of development, to common ancestry, etc., is according to Naef simply a misunderstanding. Surely, a definition such as that postulated by Naef would not interfere with any facts accumulated by comparative embryology, comparative anatomy, or the study of evolution. But we must ask whether such a definition has any real, practical value. The progress of science depends on the growing precision in the meanings of scientific terms. Definitions ought to grow continuously more precise, and not more vague. Only then can the understanding between authors who use the same words approach perfection. Therefore neither these nor the somewhat similar opinions of Kälin (1946) will in all probability receive very wide approval.

Hubbs (1944), and Haas and Simpson (1946), like de Beer (1947), see the importance of the concept of homology primarily in the use of that term in the study of animal evolution. These authors have tried to develop the line of thought of Haeckel, Gegenbaur, and Lankester. As a good example, we quote the words of Haas and Simpson: "Homology as we agree is best defined as similarity interpreted as due to common ancestry." Probably such a definition now seems the best one to the majority of biologists. But not all will agree. First, as a consequence of its acceptance, no characters resulting from parallel evolution could be called homologous. Haas and Simpson have emphasized this very clearly. But the views of Huxley (1945) that "the homology no longer implies descent from a common ancestor showing the common feature" must also be taken into account, as many biologists will agree with them, and as they are in perfect concordance both with the old concept of homology and with the modern results of experimental embryology. The definition of Haas and Simpson is also rather indefinite, and therefore the arguments put forward against Naef's definition can also be applied here. A very convincing criticism of the evolutionary concept of homology is to be found in Boyden's latest article (1947). Boyden has here pointed to the fact that in the great majority of cases our assumption of common ancestry of animals is based on the presence in the respective forms of homologous structures. We must accordingly *first* recognize the homologies and only afterwards can we attempt to find a possible common ancestor. Finally, the logical development of the purely evolutionary concept of homology must sometimes lead to a disregard for the results of related sciences, as was pointed out by Wilson (1896) and de Beer (1947) in the words quoted above: "Comparative anatomy, not comparative embryology is the primary standard for the study of homologies." Against such a point of view it may be said that no real progress in anatomy can be reached by an artificial disregard of related biological sciences. On the contrary, the development of anatomy must be based on the synthesis of the views of the anatomist, the embryologist, the physiologist, the geneticist, and others.

It seems that the most important conclusions were reached by Moment (1945). This author

thinks that the term homology has at present no value, and that it embraces in reality three different relations. On this basis, he has proposed the complete rejection of the old term homology, and the introduction instead of the word *similarity* with the addition of adjectives: homodynamic or heterodynamic (pointing to developmental forces), homogenetic or heterogenetic (pointing to the similarity of genes) and homophyletic or heterophyletic (pointing to the common ancestry). It must be clearly emphasized that Moment's proposition puts an end to the misunderstandings and difficulties resulting from the old terminology. Nevertheless, his proposition was very violently criticized by Haas and Simpson (1946). As was stated by these authors, the terms suggested by Moment have already been appropriated by various writers. Although the rules of zoological priority do not strictly apply to the terminology of concepts, it is possible that the nomenclature proposed by Moment might create further confusion. It is well known from the history of science that the introduction of new terms and concepts does not often succeed. As a striking example one can cite Mivart (1870), who introduced no less than 25 new terms, not one of which was adopted by other scientists.

It ought also to be realized that the concept of homology is extremely popular and very widely used. Nearly every contemporary biologist uses this concept as one of his principal instruments of thought. Hubbs (1944) has said that the notion of homology must be used even in comparative physiology, and has spoken of homologous and unhomologous functions. Consequently the rejection of this term would be extremely difficult.

We reach the conclusion that:

1. we cannot discard the word homology altogether;
2. neither the old definition by Owen, nor the more recent one by Gegenbaur and Lankester, is suitable for the contemporary anatomist;
3. as was first clearly pointed out by Moment, the word homology embraces three different relations and has three different meanings.

It appears from our discussion that various writers call organs or characters homologous when they fulfil at least one of the following requirements:

1. they are developed from a single organ or character of a common ancestor; or
2. they are formed by parallel evolutionary changes; or
3. they develop during ontogenesis under similar influences in a similar way.

If a student of any particular branch of zoology arrives at a conviction that one of the above enumerated relations is present among the structures studied, then he calls them homologous and is usually inclined to think that the presence of one relation of the three is quite sufficient to postulate the presence of both the others. As we have seen, that is not always true.

The way in which the concept of homology can be clarified is not to be found either in the rejection of the old and valuable term, deeply rooted in the minds of the present generation of biologists, or in the hopeless attempts to build up a new definition of the concept, which has been shown to embrace definitely divergent relations. We may hope that it will be possible to continue to use the old term, only taking care to define precisely which meaning of the term is used *in every particular case*. So, e.g., when speaking of the wing of a bird and the arm of a man, we will call them homologous organs on the assumption of common ancestry. The digital pads of anurans will be also called homologous structures, but with the annotation that the term here used refers to the parallel evolutionary changes responsible for the presence of common features. Finally, albinism or the development of bulldog-face in different vertebrates can be called homologous characters, as they are caused by identical physiological processes during ontogeny.

Naturally, in a large majority of cases the organs studied will fulfil simultaneously two kinds of requirements, e.g., the first and third, or the second and third. That, however, ought not to induce us to forget that it is not always so, and that we must sharply discriminate between the three kinds of homology.

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NEW BIOLOGICAL BOOKS

The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of Biology. In addition there will occasionally appear one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that THE QUARTERLY REVIEW OF BIOLOGY *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore, from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to H. B. Glass, Associate Editor of* THE QUARTERLY REVIEW OF BIOLOGY, *Department of Biology, The Johns Hopkins University, Baltimore 18, Maryland, U. S. A.*

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GENERAL BIOLOGY: PHILOSOPHY AND EDUCATION

THE AMATEUR NATURALIST'S HANDBOOK.

By Vinson Brown. Little, Brown and Company, Boston. $3.50. xiv + 475 pp.; ill. 1948.

The small format of this handbook (4½ x 6½ inches) suggests that it is intended to slide into a pocket for reference in the field, or is adapted for hands of juvenile dimensions. Most children, however, will have approached the end of their period of physical growth and be in high school before they will be ready for much of the text. On the other hand, a budding interest in natural history will be stimulated and guided well by means of the suggestions and references furnished. Instructions are included for maintaining a great variety of animal life in aquaria, vivaria, and cages, with clear emphasis on the need to provide comfortable quarters for each. Detailed directions are given for finding, collecting, and preserving both plants and animals, and how to follow the geological history of a region by observing the rocks and minerals. Climate is treated but star study omitted. The artificial keys to living things are broad in their coverage —including some, like ocelot and manatee, that the beginner is not likely to encounter, and carrying plants mostly to the family. The appendices include "nature tests" and a useful, classified bibliography. The forty-page index seems adequate.

LORUS J. & MARGERY J. MILNE

DIE PHILOSOPHISCHEN GRUNDLAGEN DER WISSENSCHAFTLICHEN ERKENNTNIS.

By Anton Fischer. Springer, Wien. $4.20 (paper). vi + 240 pp. 1947.

The author's fundamental assumption is that man is a biological phenomenon, but that his urge for knowledge transcends biological goals. His main purpose is to reestablish the scientist's confidence in the actuality of reality and the possibilities of real knowledge, so badly shaken by positivist relativism and its followers among modern physicists.

The author discusses the psychological and logical elements of scientific thought, the scientific method, the fundamental problems of scientific knowledge (or knowability), causality and determinism, teleology and holism, and eventually the social sciences. He gives an excellent survey of the present state of opinion in Germany, France, England, and the U. S. A. on these problems, and develops a most reasonable personal position.

This is not only a very learned and thoughtful book, but—mirabile dictu in the case of a philosophical

book written in German and by a medical man (the author being Professor of Medicine in Budapest)—it is a perfectly readable and very clear exposition of very important problems. All those who read German will not regret perusing it.

ERWIN H. ACKERKNECHT

SCIENCE ADVANCES.

By J. B. S. Haldane. The Macmillan Company, New York. $3.00. 253 pp. 1948.

This work is a sequel to *What Is Life?* recently reviewed in this journal (QRB 23: 230, 1948). The present work resembles its predecessor in consisting of a number of brief, independent essays, in having been published in a daily newspaper of a political nature, and in containing a quantity of material that appears to have been written some years ago and to be republished here without revision.

The mark of the "columnist" appears in the lack of continuity between the chapters. Their order seems to have been determined at random. The only unity given the series comes from the author's fondness for contrasting the political attitudes toward scientific research of the Soviet republics and the British Empire, to the disparagement of the latter. The average reviewer is quite likely to be so impressed by these comparisons as to overlook the fundamental significance of the work, namely, that it testifies to the fact that the British working man is far more interested in the progress of modern scientific thought than his American homolog. Consequently, in several recent notices of this book the reviewers have merely abused the author and so demonstrated their own incompetence to appraise what has been going on in Russia during the past generation.

At the time of the Russian revolution western civilization appeared to be close to its moral and intellectual nadir, and while subsequent events have disproved this, it was only natural that many prophets, Haldane among them, should have seized upon such events as the abolition of conscription, the attempted suppression of the liquor traffic, and the resort to propaganda instead of warfare to break the power of Prussian ideology as portents of hope, and should have lifted their eyes to the Kremlin in expectation of help. Their mistake was not one of interpretation, but of announcing a verdict before all the evidence was in—a chance every prophet must take.

According to Haldane, communism resembles socialism in that each member of the community is expected to contribute to its welfare to the best of his ability. The difference is that under a socialistic regime the individual's compensation is proportional to the services he renders, whereas under communism it is proportional to his needs. Therefore, socialism is a means to an end, but communism is an end in itself. But history, or so it seems to the reviewer at least, supports no such conclusion. Communism has always been resorted to as a means to an end, to be abandoned when the end has been achieved. The early Christian church tried social communism, only to abandon it and make peace with the empire after achieving social prestige. The flurries of economic communism in the nineteenth century in such places as New Harmony, Brook Farm, Economy, Zoar, Oneida, Niskayuna, Amana, and the Hütterite settlements had varied careers, but some of them had long and successful existences which culminated in the acquisition of power and wealth exceeding that of the economic matrix in which they were embedded, after which communism was abandoned because it had served its purpose.

Russian communism was political. It flourished under Lenin and Trotsky, but with the ascendency of Stalin so much political power and influence had been achieved that communism could be dispensed with as an active principle. As Haldane has pointed out, the communist party today is communist in name only; and, while Stalin has many traits of character that command Haldane's admiration, his political ideology is not one of them.

In a work of such scope, perhaps no two reviewers would select the same item for special mention. My choice is the discussion of human races and the process that formed them. The doctrine that humanity can be divided into races, some of which are superior to others, is not peculiar to the Nordics of central Europe, but is taught today in some of the English schools. Haldane rejects it absolutely and goes further. He claims that the characteristics depended upon by classical theorists as racial criteria, such as skin color and hair texture, are quite inadequate for the purpose, since they intergrade perfectly. Also there is no satisfactory way to measure such a trait as the curliness of a sample of hair. The only suitable criteria, he holds, are the blood groups, for no matter how highly hybridized modern races are there are no intergrades among the coagulating agents. In theory, a pure race would be one in which all individuals belonged to the same blood group, the presence of several blood groups in the same population being indicative of hybridization. There are no pure races in existence today, and the hybrid races which we know are distinguished only by the proportions in which the various groups occur in them. This, of course, is a very different belief from the one occasionally affirmed today, that there are no races at all.

A point of interest is the introduction of blood group B into Europe by Mongolian invaders, the first of whom were the Huns under Attila in the fifth century and the last the Tartars under Tamerlane in the fifteenth. Even Mayflower descendants may therefore owe their

B blood, like the reviewer and his wife, to a Pilgrim who was of Polish origin and presumably of mixed Mongolian descent.

SCIENCE, SERVANT OF MAN. *A Layman's Primer for the Age of Science.*

By I. Bernard Cohen. Little, Brown and Company, Boston. $4.00. xvi + 362 pp. + 7 plates; text ill. 1948.

The title of this book suggests that it is concerned only with applied science. As a matter of fact, it is a plea for more adequately endowed institutions for research in absolute science. The author maintains that any dichotomy between absolute and applied science is illogical, inasmuch as science cannot be applied to industrial or other utilitarian ends until after it has been fairly well understood from the absolute point of view. For instance, the complexity of contemporary civilization which has resulted from the application of electricity to the various situations that confront the modern way of living would have been impossible without the preliminary experimentation of Faraday, who could scarcely have estimated what a far-reaching influence his work was to have on the Industrial Revolution, even then under way. Faraday was simply an investigator who found himself in a universe which he did not understand and in possession of an inquisitive mind which impelled him to do something about it.

We may smile when we think of the medieval alchemists searching for the philosopher's stone and the elixir of life, or of the author of the letter, said to have been received only last year by the Patent Office, inquiring what things had not yet been invented, with the explanation that he was an inventor and needed this information in his business. After all, is the position of these people very far removed from that of the federal government which makes large appropriations for military research or the foundations which do the same for cancer or polio, neither of which would consider endowing an institution devoted to the pursuit of truth, whithersoever the trail might lead? X-rays were observed by Goodspeed before Roentgen, and the effect of penicillin on bacteria was observed by Fleming before Florey and Chain. But Goodspeed and Fleming were employed on specific tasks at the time and did not have the freedom to investigate what would have seemed only a side issue. Advances in science usually come about as the result of unprejudiced investigation of natural phenomena, and the failure to recognize this by those who are privileged to endow research has always been a serious obstacle to progress.

Unfortunately, the good which the author's missionary efforts might have achieved is likely to be vitiated by one weak chapter, that which gives its name to the book. Here the author has pointed out that science in itself can be neither moral nor immoral, but only amoral. From this he has concluded that the scientist must be relieved from all moral responsibility for the uses to which his discoveries may be put. But the scientist is also a human being, and cannot so easily shirk his responsibility as his brother's keeper, merely because he finds it irksome or inconvenient. The alternative would justify the use of poison gas and the sinking of unarmed merchantmen by Germany, the bombardment of the unfortified town of Karlsruhe by Britain, and the bombing of Hiroshima by the United States. The author is quite willing to whitewash these offenses, and even goes so far as to accuse those who condemn them of bad faith, saying that they are acts of war and cannot be consistently opposed by any who do not oppose all warfare. As a matter of fact, those who criticize the use of the atomic bomb are the same people who have always opposed all kinds of warfare, and by pretending that they are not the author exposes his own lack of knowledge and weakens his thesis. The book as a whole would have been considerably improved had this chapter been omitted.

The author has provided an index of six pages, a number which is rather inadequate for the quantity of material in the book. The documentation is detailed but segregated at the end of the book. The most valuable feature of the book is the list of recommended reading, thirty-two pages in length.

CONFERENCE ON INTERNATIONAL CULTURAL, EDUCATIONAL, AND SCIENTIFIC EXCHANGES. *Princeton University—November 25–26, 1946. Preliminary Memoranda—Recommendations Adopted—Summary of Discussion.*

Board on Resources of American Libraries and International Relations Board of the American Library Association. Preliminary Memoranda by Edwin E. Williams and Ruth V. Noble. American Library Association, Chicago. $4.00. xxii + 210 pp. 1947.

Probably no very great number of scientists will read this book—the more's the pity, for it deals with problems of vital importance to science. In the sciences alone, thousands of journals annually publish hundreds of thousands of scientific articles. Libraries can no more keep pace with this flood of print than a scientist can cope with the volume of published matter in his own field. Indexing and making available to those who need them the particular publications desired become crucial to further progress. The breakdown of scientific exchanges because of war and the destruction of libraries in many places have further aggravated the difficulties of the present world situation.

This book contains the considerations of the con-

ference on twelve topics: Author Bibliography; Subject Bibliography; Interlibrary Loan; Photographic Reproduction; Cooperative Acquisition and Specialization; International Exchange of Documents; Exchanges between Libraries; International Commercial Exchanges; Barriers to International Interchange of Publications; Exhibits; International Exchange of Personnel; and Rehabilitation, Agencies and Priorities. The 24 unanimously adopted Recommendations of the Conference deserve the attention and support of all scientists.

BENTLEY GLASS

FUNDAMENTAL EDUCATION. *Common Ground for All Peoples.*

Report of a Special Committee to the Preparatory Commission of the United Nations Educational, Scientific and Cultural Organisation, Paris 1946. The Macmillan Company, New York. $2.50. viii + 325 pp. 1947.

The Education Section of the Preparatory Commission of UNESCO has undertaken in this preliminary report to define the scope of "Fundamental Education" and to present some of the experiences and problems derived from existing campaigns against illiteracy. The analysis of problems and issues leads to a further definition of Policies and Methods (chapter 4), and from these come forth the Suggested Lines of Action (chapter 5). Who is to be educated and how? What is to be the content of fundamental education beyond mere literacy, which all agree is not enough? What should be the relation between the State and voluntary agencies? What language should be the basis? How may reading materials be provided? These are samples of the questions raised and considered, although, naturally, not fully answered.

It is proposed that a panel of about 15 persons of different backgrounds should be formed, including a sociologist or anthropologist and a psychologist. This panel should undertake "thorough documentation" of the field, "planning of staff services of information," "personal contact with workers in the field," and "direct study of a variety of problems in Fundamental Education." If the present state of the entire undertaking is rather nebulous, it is at least a beginning; and the book itself provides an interesting look at our new international machinery in action, in a field of basic importance to all learning, including science.

BENTLEY GLASS

WIDENING HORIZONS IN MEDICAL EDUCATION. *A Study of the Teaching of Social and Environmental Factors in Medicine. A Report of the Joint Committee of the Association of American Medical Colleges and The American Association of Medical Social Workers. By Jean A. Curran and Eleanor Cockerill, Co-Chairmen. The Commonwealth Fund, New York; Geoffrey Cumberlege, Oxford University Press, London.* $2.75. xiv + 228 pp. 1948.

This report appropriately starts out: "The patient comes to the hospital with his medical complaints, but leaves his environment at home." It has been increasingly realized that something has to be added to the biological and hospital training of the future doctor if he is to be fully equipped for the practice of medicine. A joint committee of the Association of American Medical Colleges and the American Association of Medical Social Workers has studied, between 1945 and 1946, what is actually being done to impress the medical student in American schools with the roles of social and environmental factors in medicine. It here submits its results and suggestions.

Some schools do an excellent job. But only one-third of American schools (actually probably less, for a report of an activity is not always identical with its realization) did something along these lines in 1945–46. Where such training is given, it is limited to the clinical years, and no provision is made for the preclinical and premedical years. The most common method of teaching social and environmental implications seems the case study by clinical students, with the cooperation of the social workers. Numerous examples of this obviously effective method are given.

This is a very important problem, and those concerned with it will find valuable suggestions, bibliographies, and good ideas in this report, although it suffers from the literary defects inherent in most "reports" and the limitations imposed through the "transition" period (1945–46) in which it was composed.

ERWIN H. ACKERKNECHT

WHITHER MEDICINE: *From Dogma to Science? By Antony Fidler. Thomas Nelson and Sons, London, Edinburgh, Paris, Melbourne, Toronto, and New York.* 6s. xii + 115 pp. 1946.

The author's statements that our present day medicine is based on the idea of causality, that very often (the author says "always") it falls short of being truly causal, and that practically we operate often with probabilities instead of true causes, are true. It is equally true that medicine is inconsistent in referring separately and by turns to anatomy, or physiology, or bacteriology. (It might be added that it has fared considerably better with the inconsistencies of the last 100 years than with the consistencies of the previous millennia.)

The whole problem is whether we therefore ought to make the radical change proposed by the author, to adopt his new, consistent "medicine of probability,"

based exclusively on clinical statistics, to throw out "materialistic biology" and all the rest; or whether we should try to improve upon our present ways. On the basis of past experience, the arguments of the author, and the samples of his new method, the latter is recommended.

Many of the critical arguments of the author will be profitably studied by those who have never examined the philosophical foundations of our present day medicine, although the whole discussion is not very new. A thorough discussion of the author's arguments, from the point of view of the biologist, can be found in Claude Bernard's *Introduction into Experimental Medicine* (1865).

ERWIN H. ACKERKNECHT

BIOLOGY: HISTORY AND BIOGRAPHY

A SOURCE BOOK IN GREEK SCIENCE.
By Morris R. Cohen and I. E. Drabkin. McGraw-Hill Book Company, New York, Toronto, and London. $9.00. xxii + 579 pp.; ill. 1948.

Most contributions that deal with the history of Greek science have been written primarily for the specialist and few scholarly works are available that give a panoramic picture of the whole field of ancient scientific thought and achievement. Furthermore, much of the vast literature on the ancient Greek scientific efforts is dispersed among philological books and periodicals not readily available to the average student. There is, therefore, a definite need for a book giving a range of primary sources in the various disciplines of the natural sciences for which more precise references may be found. This contribution of the late Professor Cohen and I. E. Drabkin appears to fill such a need, and it should have a special appeal not only to the biologist and other students of the natural sciences but to the cultured layman as well.

It appears that the authors have been needlessly concerned in trying to draw a fine line between what may be considered truly representative of ancient Greek scientific observation and what is folklore. Future historians who wade through our own specialized scientific journals will doubtless be beset by similar difficulties of discrimination and so probably will be *their* historians. The difference between sense and nonsense in such matters is a relative one. The now seemingly absurd conclusions sometimes arrived at by even such scholars as Aristotle, Theophrastus, Ptolemy, and Galen are not quite so ridiculous when it is borne in mind that they were made on the basis of the information available at the time. It is a simple matter today to classify the work of the ancients into science or superstition, with the 2,000 or more years of accumulated information we have at our disposal. The amazing thing is that they did so well on so little. Viewed in this perspective, there is little indication that man's capacity to interpret data has increased, and what appears today as an improvement in this function is only an apparent one. Man simply has more factual information to work with.

The general idea that the scientific method began only three or four hundred years ago, and that the ancient Greeks were mere armchair speculators or philosophers given only to qualitative thinking, is certainly in error. If we consider the collected works going under the name of Hippocrates of Cos, the treatises of Galen, or the writings of Oribasius or Theophrastus, there is much evidence that the scientific method of inquiry began at least in the 5th century B.C., and probably earlier. Fragmentary data seem to reveal that Praxagoras, Erasistratus, and Herophilus certainly used the experimental method in their studies on brain, spinal cord, eye, and vascular system. Erasistratus of Ceos, a contemporary of Herophilus at Alexandria (around 300 years B. C.), apparently distinguished between motor and sensory nerves. There are also indications that he employed quantitative experimentation on metabolism almost 2,000 years before Sanctorius. Still later, Galen (A.D. 130–200) reported his observations on the results of sectioning the spinal cord at various levels.

The contents of this book have been grouped under the following modern categories: Mathematics, Astronomy, Mathematical Geography, Physics, Chemistry, Geology and Meteorology, Biology, Medicine, and Physiological Psychology. This arrangement facilitates finding material that may be of particular interest to various specialists. Although the passages are abstracted from their contexts, the authors have overcome this disadvantage by a generous use of explanatory footnotes.

This book is unhesitatingly recommended as an addition to the personal libraries of all those interested in the progress of the natural sciences. It undoubtedly will be on the shelves of all modern libraries.

DAVID B. TYLER

AETIOLOGICAL PRINCIPLE OF PYAEMIA IN ANCIENT EGYPTIAN MEDICINE. *Supplements to the Bulletin of the History of Medicine. Number 10.*
By Robert O. Steuer. The Johns Hopkins Press, Baltimore. $1.50 (paper). viii + 36 pp.; ill. 1948.

This very interesting but highly technical monograph is devoted to the Egyptian medical term WHDW. The term seems to have stood for an etiological principle that in the living person acts in the intestine and corrupts the blood, and in the dead brings about decomposition.

ERWIN H. ACKERKNECHT

EMANUEL SWEDENBORG: *Scientist and Mystic.* *By Signe Toksvig. Yale University Press, New Haven.* $5.00. x + 389 pp. + 4 plates. 1948. Emanuel Swedenborg was probably the greatest genius in all history, with the possible exception of Leonardo da Vinci. These two resembled each other in their mechanical abilities, but differed in that whereas Leonardo's surplus energy manifested itself in artistic creation, Swedenborg's went into religion and philosophy. Emerson, in his essay on "Representative Men," selected Swedenborg as the representative mystic, and in his Phi Beta Kappa oration expressed a similar thought. On the other hand, Evelyn Underhill has denied Swedenborg's claim to be called a mystic at all. This seems strange, since Swedenborg's death was less than two centuries ago, and the facts of his life are well attested. Legends have not yet grown up about him as they have about some of the great mystics of the dark ages, whose biographers have difficulty in separating wheat from chaff.

Thanks to this biographical work by Signe Toksvig, the reader who has been confused by the contradictory attitudes of these two critics can now familiarize himself with the facts and draw his own conclusions. To me it appears that the disagreement lies in the definition of terms. To Miss Underhill mysticism is a sense of communion with a higher power, while to Emerson it was a manifestation of superconscious phenomena. To both it connoted something occult.

That Swedenborg did on several occasions manifest apparently occult powers is well documented. The best known of these instances concerned the conflagration in Stockholm. Although more than one hundred miles away at the time, he announced the oubtbreak of the fire and gave the correct locality as soon as it occurred, stated correctly that his own home had been threatened but had escaped injury, that the house immediately adjoining had been damaged, and finally that the fire was under control. On another occasion, the queen of Sweden asked him to communicate with the spirit of her brother, who had died recently. About a week later Swedenborg reported to her that he had made the contact successfully and that her brother had apologized for not having replied to her letter. Now it so happened that the queen's brother was a member of the Austrian nobility, and Sweden was at war with Austria at the time. In writing to him at all the queen was guilty of communicating with the enemy. Naturally she did not wish this known and there is no reason to doubt her statement that no one knew of this letter.

Again, Swedenborg's aid was sought by a widow who asked him to locate a receipt for a payment made by her husband, for which she was being dunned, the creditor having claimed that the payment had never been made. Swedenborg made contact with the deceased husband and reported that the receipt would be found in a certain drawer of a bureau on the second floor of the widow's home. The widow stated that she had already cleared the bureau. Swedenborg replied that she had missed a secret drawer, of whose very existence she was not aware. Examination of the bureau revealed the drawer and disclosed the missing receipt.

These occurrences cannot be dismissed as fabrications, for all of them were investigated within a few years by no less a person than Immanuel Kant, who interviewed the witnesses and pronounced the phenomena genuine, though of course unable to offer any explanation. Later, F. W. H. Myers of the Society for Psychical Research suggested a telepathic explanation to account for them, but it must be confessed that the possibility of telepathy in the case of the lost receipt seems rather remote. Possibly some readers may feel that a telepathic explanation might be more occult than a spiritualistic one. There can be no doubt that Swedenborg himself believed that he had been in actual communication with the departed.

The emphasis placed on Swedenborg's psychic experiences in this review should not be construed as indicating that they were the most important phase of his character, but they are certainly the most outstanding. Swedenborg's dissection of the human brain, together with his observations on the process of cerebration, have become the basis of modern brain surgery; but there have been other great anatomists, other mining engineers, other bridge builders. But psychically Swedenborg stands alone. Many scientific men have been deeply religious, such as Moses Maimonedes, Nicholas Copernicus, and John Ray. But these were not mystics. The great mystics of history have not been scientific men, and it is difficult to appraise their testimony. There have been exceptions, of course. Nicholas Cusanus was both a scientific experimenter and a mystic, but he is so remote in time that our knowledge of him is quite meagre. Blaise Pascal seems to have been diverted from the field of science to that of mysticism by a religious experience, and the personality revealed in the treatise on conic sections seems quite different from that revealed in the *Pensées*. But when Swedenborg became a mystic he did not cease to be a scientist. He writes of the discarnate spirits as if they were the inhabitants of a foreign country in which he was travelling. Although they have no physical bodies they are not aware of this, and it was necessary for him to become their teacher and adviser in order that they might become oriented to their new surroundings.

Swedenborg tells us that the disembodied spirits do not exist in a universe of space and time, although they think they do. Space and time, he tells us, are a form of sense perception. Those who think the same

thoughts perceive each other as nearby; those who think different thoughts about the same object perceive each other at a distance; those who think about different objects do not perceive each other at all. This seems like an adumbration of the philosophy of a writer, whose name eludes the reviewer's memory, who claims consciousness to be a dimension in the space-time continuum. Another surprising adumbration in Swedenborg's thought is the theory that the human mind is triune. Its lowest part governs reflex actions and is unconscious. Above this is another entity which controls volition and emotion and is conscious. On the highest level is a supermind which exercises moral judgment. In this belief he anticipated Freud, but he was unlike Freud in that the latter believed that all dreams have a sexual interpretation, whereas Swedenborg believed that even dreams of a sexual nature had an interpretation entirely independent of sex.

Swedenborg's formal education was at the University of Upsala, under Celsius and Rudbeck. Nineteen years later these same two had among their students another who was destined to exert a profound influence on world thought. Yet in this biography the name of Linnaeus is not once mentioned. Nor, I believe, does the name of Swedenborg occur in the definitive life of Linnaeus by the president of the Linnaean Society. It is difficult to suppose that the two greatest intellects of Sweden never met, yet such seems to have been the case. Perhaps the difference in their ages was responsible, or the fact that Swedenborg lived so much of his life in London, or perhaps a difference in temperament kept them apart. Linnaeus was popular with youth. He was always surrounded by a group of young enthusiasts, who eventually carried on to a successful conclusion the work which the master left unfinished. Linnaeus lived again in the lives of Fabricius, Hasselquist, Rotherham, Kuhn, Sparmann, Loeffler, Solander, Kalm, Zoega, Thunberg, and Forskål. But Swedenborg had no apostles. It is true that a large body of Christian believers bears his name, but Signe Toksvig tells us that it was not his intention to establish a new sect. In this respect he was like George Fox and John Wesley, yet with a difference: Fox and Wesley are so identified with the movements which they started that it is impossible to study them without at the same time studying Quakerism and Methodism, but there is no such intimate connection between Swedenborg and Swedenborgianism. In fact, one of Swedenborg's followers has informed this reviewer that there are in reality two Swedenborgian churches whose beliefs differ somewhat strikingly.

Swedenborg died as he lived—a mystery to others, if not to himself. We cannot understand him as a whole, but we can understand certain phases of his character. He is not to be explained or accounted for; he can only be accepted.

FONTES HISTORIAE BOTANICAE ROSSICAE. *Chronica Botanica, An International Collection of Studies in the Method and History of Biology and Agriculture. Volume 11, number 2.*

By Vladimir C. Asmous. Chronica Botanica Company, Waltham, Mass.; G. E. Stechert & Co., New York. $1.25 (paper). Pp. 87–118 + 4 plates; text ill. 1947.

In this useful bibliographical work the author has undertaken to enumerate all publications, except those of a general and worldwide coverage, which deal with the history of botany in Russia. Biographical and bibliographical publications have been included along with the strictly phytohistorical. A brief characterization and evaluation of each entry is added, the author having personally examined by far the greater number of them.

THE HUXLEY PAPERS. *A Descriptive Catalogue of the Correspondence, Manuscripts and Miscellaneous Papers of the Rt. Hon. Thomas Henry Huxley, P. C., D. C. L., F. R. S., preserved in the Imperial College of Science and Technology, London.*

By Warren R. Dawson. Macmillan and Company, London. 25s. xii + 201 pp. 1946.

To every biologist and literary historian interested in Thomas Henry Huxley this careful classification of the major collection of his papers will be indispensable. Sixteen categories of papers fill 83 volumes. Of these, 30 fall under the head of Scientific and General Correspondence, the others under Supplementary Letters, Personal Papers, Anthropology and Ethnology, Biology (vols. 34–41), Education, Fisheries, Geology and Paleontology, Philosophy and Ethics, Theology and Biblical Criticism, etc., Various Subjects, Notebooks and Diaries, Drawings, Caricatures and Cartoons, Photographs and Engravings, and finally Posthumous Papers. Notes on the character of the contents of each entry are illuminating.

The general reader will be most interested in the comments of the indefatigable editor, comments which reveal a good deal about Huxley's ways:

> The difficulties of this laborious undertaking were greatly increased by the paradoxical fact that Huxley, though essentially a man of science, so far neglected the principles of order and accuracy inherent in the scientific method, as to be utterly unsystematic, unbusinesslike, and careless in all that concerned his correspondence and papers. His handwriting was atrocious and my attempts to discern the purport of his papers have often become exercises in cryptography rather than in reading. His letters and papers written before and during the voyage of the *Rattlesnake* are usually fairly neat and legible, but from 1851 a rapid deterioration set in which became more and more pronounced with the years. He scarcely ever headed, docketed or dated his papers: and even when he did date them, the dates were generally expressed by a single

sprawling ligature . . . that might mean almost anything. Many of his notes and drafts are well-nigh illegible and only a long acquaintance with the peculiarities of his script has enabled me to decipher them. Huxley's handwriting was a frequent source of complaint by his correspondents and it was at times, as he has confessed, a severe trial even to himself. He was also equally careless with the filing of his papers, for from nearly all his manuscripts sheets are missing or transposed and the pages are often incorrectly numbered.

And as to his literary composition:

The materials for the first lectures of a series are usually very full, but the notes diminish in length and number as the course proceeds . . . . Mitchell observes: "His literary style, his brilliant rhetoric and acute disputation came to him slowly: they were the outcome of laborious effort and continual practice." This statement was challenged at the time, but there can be no doubt as to its truth. Of his oratory, Huxley has himself confessed in his *Autobiography* the struggles he had to make in order to acquire that fluency which afterwards became so characteristic of his speeches. And as to his powers of writing, his papers show that it entailed equal, if not greater, difficulty. We . . . are apt to assume that the words flowed from his pen in writing as they flowed from his lips in speaking at the zenith of his carreer. His manuscripts, however, show that the facts of the case are far otherwise. Throughout his life he seldom wrote a dozen lines without cutting up his text—altering, deleting, or interpolating words and phrases in almost every sentence. And when at last he was sufficiently satisfied with his copy to send it to the printer, the arrival of the proofs signalized the onset of another attack on his prose . . . he at times almost rewrote his articles when in proof.

There are also interesting notes about Huxley's philosophical and theological interests during the later part of his life, but space forbids more quotation. This is enough to show, at any rate, that the catalog is far from a dull and uninteresting compilation.

BENTLEY GLASS

HISTORY OF FACTORY AND MINE HYGIENE.

By Ludwig Teleky. Columbia University Press, Morningside Heights, New York. $4.50. xvi + 342 pp. 1948.

This first history of factory hygiene, a subject that concerns so many medical and non-medical people, will be most welcome to a large group of readers. The author starts in by giving the prehistory of industrial hygiene as a science (from antiquity to the middle of the 19th century). He then switches to the history of practical fields like factory legislation, organizations cooperating in industrial hygiene, accident and industrial disease prevention, etc. In chapter 8 he takes up again the history of industrial hygiene as a science, giving particular attention to dust diseases, caisson work, occupational skin diseases, and mining. A last chapter tries to assess the effects of industrial hygiene.

An extraordinary amount of valuable thought and information is compressed in this relatively small volume. Though handling historical data very ably, the author is no "professional" historian, and this together with the limitations of space, has probably induced him to lay emphasis on the more recent past, just as technical reasons have compelled him to concentrate on England, Germany, and the U. S. A. This will probably be regarded by many as an advantage rather than a disadvantage. In any case, industrial hygiene is for the most part a very young science. The author's own active professional life of almost fifty years in the first ranks of industrial physicians covers a good deal of the history of industrial hygiene, and enables him to speak with a vividness and practical understanding about matters that in other hands might, in spite of their intrinsic interest, have become dull and impersonal. An excellent bibliography partly compensates for the absence of reference footnotes.

Statistics, revealing a marked decrease in industrial accidents and diseases in all civilized countries, show that the painful exertions of physicians, enlightened manufacturers, lawyers, unionists, etc., during the last one hundred years have not been in vain. The same statistics unfortunately also show, as emphasized in a preface written by our own Alice Hamilton, that while we have done very well in the field of research in this country, we fall, due to poor legislation, far short of the more advanced European nations in the actual prevention of accidents (especially in mines) and of industrial diseases.

ERWIN H. ACKERKNECHT

ANT HILL ODYSSEY.

By William M. Mann. An Atlantic Monthly Press Book, Little, Brown and Company, Boston. $3.50. xii + 338 pp. + 4 plates; text ill. 1948.

The most important mammal in any zoological garden, and the one of which most visitors know least, is its director. Our National Zoo in Washington has few rivals to-day, chiefly as the result of the well planned guidance of its present director, of whom the reader will learn a lot in this delightful volume.

The author of this autobiography was born in Montana at a time when, nearby, the late director of the Bronx Zoo, Hornaday, could still find plenty of buffaloes from which to shoot the famous group that is on display in the National Museum. In these reminiscences, Mann describes his most romantic, earlier years, but closes abruptly when his professional career is assured by the offer of a full-time appointment in the Department of Agriculture with the "incredible salary of eighteen hundred a year." That was, he confesses, "one of the few times in my life I was

speechless." This ends the volume, yet it is only the beginning of the author's mature and successful life, crowded with unusual experiences, connected with nearly everything in the animal kingdom from ants to men and ranging from Washington to Liberia and Singapore. So we must hope for a second volume of this autobiography to describe the due metamorphosis of the enthusiastic student of ants into the foremost "gardener" of an entire, glorious zoo, and to reveal the later stages in the development of an expert in handling such varied creatures as penguins, aard-varks, and members of appropriation committees.

The author's absorbing passion for studying and collecting animals is manifest on every page of this book. When he was but 12 years old he had made himself "two wonderful aquaria" with placer pans which just fitted the two holes of an old outhouse. Still a schoolboy, Mann spent a vacation at the National Zoo, cleaning cages under the tutelage of Mr. Blackburne, who was still the grand old headkeeper for many later years under Mann's directorship. Incessantly collecting bugs and snakes and trading rare insects with expert entomologists, the young Mann became acquainted with many leading naturalists of the time. After having been sent by one of the latter to Arizona for beetles, and having taken part in a Stanford expedition to Brazil, collecting ants and wasps, Mann went to Harvard as a student and entomological assistant under the late Prof. W. M. Wheeler. The laboratory, however, could not compete with the lure of distant countries. Accepting avidly every chance to travel and thereby postponing repeatedly his doctor's thesis, Mann went to Haiti for ants and snakes, to Mexico for moths, to Arabia for insects and reptiles, and to remote Pacific islands for everything zoological he could collect. The account of all these fascinating trips with their many adventures and the thrills of finding new or rare species will be enjoyed by every true naturalist. Comments on ants and their ways are scattered among innumerable good anectodes of head-hunters, missionaries, cannibals, Arabs, and scientists, who invariably were the author's friends. Here is a most readable story of an enthusiastic young zoologist and world-traveller who got what he went after in spite of all obstacles, who saw the humorous side of everything, and who never complained of hardships or bad luck.

A. H. SCHULTZ

FIELDING H. GARRISON. *A Biography.*

By Solomon R. Kagan. The Medico-Historical Press, Boston. $4.00. 104 pp. + 4 plates. 1948. The late Colonel Fielding H. Garrison was destined by nature to be a musician or mathematician. Circumstances made him for 39 years a bibliographer in the Army Medical Library in Washington, the biggest medical library in the world. Garrison resolved the conflicts of this situation by becoming one of the outstanding medical historians of all times, and the greatest this country has produced so far.

To write a biography of Garrison, who led an externally uneventful, internally tragic life is extremely difficult. The author, who has already published a *Life and Letters of Garrison* in 1938, has the enthusiasm, industry, and gift to dig out the materials needed for this task. Unfortunately, his style and his ability to arrange and to analyse material are not quite up to the same level. Admirers of Garrison will nevertheless be extremely grateful to Kagan for the new and significant data that he has brought to light, especially in regard to Garrison's youth and family background. This volume contains a considerable additional number of Garrison's rightly famous private letters, unpublished until now.

ERWIN H. ACKERKNECHT

EAST OF THE ANDES AND WEST OF NOWHERE. *A Naturalist's Wife in Colombia.*

By Nancy Bell Bates. Charles Scribner's Sons, New York and London. $3.50. 237 pp.; ill. 1947. Since the sixteenth century, explorer-naturalists have found Central and South America to be fertile areas for scientific investigation. The natural history of these countries has been chronicled in the writings of such men as Darwin, Wallace, Bates, Belt, Hudson, and Beebe. It may come as a surprise to many readers to learn that South America now attracts the laboratory-trained scientist as it once attracted the field naturalist. Here is the story of the life and research work of a scientist stationed in Villavicencio, Colombia. It is a story that differs from the usual account in that it is written by the wife of a scientist. You will find in Mrs. Bates' writing a charming, conversational, and frequently amusing touch which portrays the pleasures and problems of life in the llanos, the vast grassy plains bounded by the Andes on the west and the jungle on the east. Much of the charm of this book lies in the fact that it is written from a layman's point of view but by one intimately associated with the laboratory's program of research on yellow fever. This book is filled with anecdotes of everday life in one of the little known areas of the world. Interspersed are descriptions of the people, the animals, and the country to be seen on the numerous journeys to, from, and about Villavicencio. Numerous photographs taken by the author and her husband complete the picture.

V. G. DETHIER

ECOLOGY AND NATURAL HISTORY

NATURE'S YEAR.

By Maribel Edwin. Longmans, Green and Company,

London, New York, and Toronto. $2.50. 128 pp. + 55 plates; text ill. 1947.

Simple descriptions of the manifold interrelationships of plants and animals carry the reader of this book through the cycle of the seasons. The style is suitable for those of junior high school age. By far the most outstanding feature of the book is the collection of beautiful photographs, excellently reproduced, that illustrate the text. This is natural history in its local setting and modern ecological dress. Although written for the young amateur of biology in Great Britain, it will fit the North American setting almost as well. A better introduction to biology could not be wished.

BENTLEY GLASS

LA FORME DES VÉGÉTAUX ET LE MILIEU. *Collection Armand Colin (Section de Biologie).*

By Raoul Combes, with preface by M. Molliard. Librairie Armand Colin, Paris. 80 fr. (paper). ii + 222 pp.; ill. 1946.

This small volume, published in French, contains a review of the effects upon the forms of plants that come about with changes in environment. The first chapter deals with the degree and kinds of plasticity that occur in plants—their variability in physiological processes, external and internal structure, and developmental characteristics. Chapters 2 to 5, inclusive, contain discussions of the effects of such environmental factors as temperature, light, water, and chemical agents. A final chapter treats of the effects of certain environmental complexes—the aquatic habitat, the littoral climate, and the alpine climate.

The book contains, interspersed throughout the descriptive matter, brief but significant accounts of earlier work. The discussions seem to be relatively free of teleology, and to present a good general survey of the field. There is a list of cited literature at the end, and a table of contents. Illustrations consist of line drawings in twenty-six text figures.

H. M. RAUP

L'ÉRABLIÈRE LAURENTIENNE. *II. Les successions et leurs indicateurs. Contributions de l'Institut Botanique de l'Université de Montréal, Number 60.*

By Pierre Dansereau. Institut Botanique, Université de Montréal, Montréal. 50 cents (paper). Pp. 235–291; ill. 1946.

This an ecological description of the deciduous forests of the St. Lawrence valley, particularly relating to their mode of development. The climax tree of the area is considered to be the sugar maple, *Acer saccharophorum* (*A. saccharum* of most manuals). It is thought to be developing climax forests at the ends of several sequences that have diverse origins and varied components. On the aquatic side, one sequence is thought to lead through a bog type involving *Chamaedaphne* and *Kalmia*, while another involves a wet meadow of *Calamagrostis* with willow margins. On the xerophytic side, the principal successions appear to lead through grassland and involve either white pine or birch in the course of their development. A fifth successional source for the climax is thought to be in cedar swamps of *Thuja occidentalis*.

The existing forest types of the valley are all regarded as stages in the natural development of the climax, or stages in arrested successions resulting from unusual physiographic or climatic conditions, or the result of disturbance of natural successions by man. The types recognized by Dansereau are correlated, wherever possible, with those of earlier students.

The methods of description and analysis for plant communities are modeled after those of the "Swiss School" of Braun-Blanquet. The concept of succession used is clearly that of Clements. The involved nomenclature of communities, also derived largely from Braun-Blanquet, will be confusing to many American students. It is based upon the thesis that there should be a latinized binomial or trinomial nomenclature for plant communities to facilitate the spread of knowledge among botanists of different countries; and this in turn rests upon the idea that a community is floristically a sufficiently describable entity to be analogous to a species. Should the latter idea be seriously questioned, as it commonly is, then the usefulness of any standardized nomenclature is open to serious question as well.

Dansereau's best evidence of actual successional change is in his studies of sequences that have been disturbed or created by man's interference—those in which visible development has occurred within the memory of living men or during the recorded history of the St. Lawrence valley. The idea of succession can be projected far into the past, but the further one goes back into the development of primary, natural successions the more difficult it becomes to establish actual "genetic" relationship between adjacent communities. This is particularly true in the youthful vegetations of the glaciated regions. One wonders, for instance, whether it is possible to say with confidence that an association predominantly of *Chamaedaphne* is developing toward one of *Kalmia augustifolia* which is adjacent to it, or that it is developing at a faster rate toward some other association as yet unrepresented in the bog, or that it is scarcely changing at all and is merely the vegetational expression of the current habitat-plant relations. This reviewer has studied many lake- and pond-shore community complexes that gave every appearance of successional sequence but which proved to be only temporary juxtapositions of communities, made and destroyed by geomorphic events that were running in unpredictable periods or cycles. The identification of actual successions that can satisfactorily be documented becomes, therefore, an exceedingly difficult task in many regions.

Dansereau raises these questions, but apparently feels that the general trends in the development of his maple climax are sufficiently clear to render of small consequence any uncertainties as to detail.

The paper appears to be painstaking and thorough. It is well-organized and adequately illustrated with diagrams and tables. It closes with a short glossary of the principal ecological terms used, a brief summary in English, and a list of cited literature.

H. M. RAUP

DIE FLAMINGOS DER CAMARGUE.

By Etienne Gallet; foreword by H. Hediger. Werner Krebser, Thun. 127 pp.; ill. 1949.

Where the Rhone empties into the Mediterranean is an extensive wasteland of brackish marsh inhabited almost exclusively by flamingos. Because of the extreme changes in water level, ranging from complete submersion during the winter to almost complete desiccation during the summer, relatively little vegetation and even fewer animals have been able to survive there, with the exception of these birds. The author studied these birds over a period of 15 years, and his numerous observations are recorded in this book, translated from the French by Hans Donau.

In the spring the flamingos are extensively scattered over the Camargue and are to be seen in large flocks feeding by a peculiar circular motion of their necks around their legs. The mud is dug and strained by the bent, lamellated bills resulting in a circular moat around the standing bird. Nesting activities soon follow, and the odd mud mounds used as nests are fashioned by the females. The final shape of the nest is determined in large part by the nature of the substratum. The sun bakes the clay into a firm raised platform upon which a single egg is laid. In two per cent of the cases there were two eggs, but the author was not able to determine whether more than one female had laid them.

After 30 days of incubation, by both parents, the chick hatches, with straight bill and short stout legs. Close observation of the feeding indicated that a clear fluid is regurgitated by the parent, likened by the author to pigeon milk, but for which there is no comparable anatomical basis in the flamingo. This diet is believed essential for the chick until it is old enough to tolerate salt in large quantities. When returning from their feeding grounds, the adults apparently recognize their own nests, but not their chicks. The parents will tolerate any single young near the nest, whether its own does not matter, so that a form of community nursery exists.

By the time the water level is receding, the young are old enough to enter the water and feed themselves. No living organisms exist in the salty medium, however, and repeated examinations of the flamingo feeding areas revealed nothing but sand and mud which contained approximately eight per cent organic matter. All other forms of aquatic birds leave the area at this season. Stomach analyses reveal nothing but the mud, so that the author has been forced to conclude that the birds subsist wholly on the organic content. Later in the season, when most of the Camargue is a crystalline saline wasteland, the birds concentrate at Vaccarès, where complete desiccation does not occur.

A swelling of the crop in young birds was frequently noticed. Subsequent autopsies revealed this to be caused by an abundant flow of blood into the lumen of the crop. After several hours the blood was resorbed, although some chicks were apparently unable to recover and died. It is not known whether this curious phenomenon is natural or a pathological condition resulting from K avitaminosis.

The major portion of the book describes all of these and many other aspects of the flamingo's life history. An additional section of the book, entitled Supplementary Observations, gives in condensed form many of the data discussed previously, plus additional quantitative data on size, weight, and population totals. The latter would indicate that the birds are not diminishing (since 1914), although the author holds that their destruction of the vegetation is gradually altering the habitat into one unsuitable for the continued existence of the birds.

Not knowing what conditions for printing exist abroad, but certainly feeling cheated by the raised prices and poor quality of recent textbooks in this country, the reviewer was surprised at the beautiful make-up of this book, which has large clear type, heavy paper, attractive binding, and superb illustrations. Some 53 plates of the area, the nests, the chicks, the adults, all in various phases of their activities, form one of the most beautiful pictorial records of flamingos that has ever been assembled.

The major deficiency of this volume seems to be one of omission. One gets the feeling that during all those years of observation more information was obtained than has been placed between the covers. Furthermore, no mention is made in text or bibliography of Chapman's work on the American flamingo. This is unfortunate, for even though the American flamingo is a different species from the European, the two are similar enough for comparisons to be valuable. Thus, Chapman also observed the liquid diet fed the young birds, but found that it consisted of the regurgitated essence of the mollusc *Cerithium*. The latter was found to be the sole staple in the adult's diet. However, Gallet's artistic contribution to the life history of one of nature's most bizarre creations is well worth reading.

HENRI C. SEIBERT

EVOLUTION

THE EVOLUTION OF GOSSYPIUM *and the Differentiation of the Cultivated Cottons. Empire Cotton Growing Corporation.*

By J. B. Hutchinson, R. A. Silow, and S. G. Stephens. Geoffrey Cumberlege, Oxford University Press, New York, London, and Toronto. $4.25. xii + 160 pp.; ill. 1947.

A very slender little book, this nonetheless unquestionably takes its place among the most important books on plant evolution. No student of evolutionary processes can afford to be less than thoroughly acquainted with the facts presented here and also with the authors' lines of thought.

Part One, by Hutchinson, deals with the Classification of the Genus 'Gossypium.' Apart from providing the necessary background for understanding the evolutionary concepts developed later, the author has undertaken a revision of the genus which reduces the species to 20 in number, grouped into 8 sections. These species are fully described, along with the principal included varieties (formerly species). The conclusion is worth quotation:

> The outstanding feature of the taxonomy of *Gossypium* is the way in which diversification has accompanied geographical distribution. The differentiation of the main Sections is associated with their distribution into the four tropical and subtropical continents. Further development was of minor importance until the invention of spinning by man, since when the two Sections bearing lint have had enormous selective advantage over the rest of *Gossypium*. The vast increase in numbers in these Sections consequent on domestication, their introduction by man into new environments, and the particularly favourable conditions provided by cultivation for adaptive evolution under natural and human selection, have given rise to the great range of forms now found in the cultivated cottons.

Part Two, The Evolution of the Species of 'Gossypium' (Hutchinson and Stephens), considers first the geographic distribution of the wild lintless species and their relationships to one another (New World, Australia, and Afro-Asiatic). Then the origin of the cultivated cottons comes up. There is evidence that both the basic change from lintless to linted and the change from simple seed hairs to convoluted lint hairs are genetically simple, although subsequent polygenic modification of the genetic system occurred around these primary changes. The lintless species show great specific differentiation and limited distribution, whereas the linted species are limited in specific differentiation, show great genetic variability, and enormous geographic range. Yet the only constant difference between the former and the latter is in the possession of lint. The conclusion is that "the origin as well as the distribution of the cottons is intimately connected with the utilization of their lint by man." This conclusion is exceedingly well documented by the authors. The region of origin is placed in the Indus valley, the time at 3000 B. C. or earlier.

The polyploid New World cottons have already been shown by Skovsted to be allopolyploids derived from a hybrid between an Old World cotton and a wild American species. Harland and Atteck, and later Silow have added to the evidence by demonstrating that duplicate gene loci and duplicate linkage groups exist in these polyploids. The main question, then, is that of the source of the Old World cotton involved. By ingenious reasoning, both biological and ethnological data are assembled to support the view that the allopolyploids originated under cultivation in the mountain valleys of northwestern South America. "The great strength of the theory lies in the fact that not only are the main biological and ethnological arguments independent, but the biological argument is itself composed of a series of independent lines of evidence. With so many inquiries yielding the same answer, there is scarcely room for doubt that the cottons of the New World as well as those of the Old owe their origin to the activities of civilized man."

Part Three concerns The Differentiation of the True Cottons, and is by Hutchinson and Silow. This, both for Old World and New World cottons, is a complex, interwoven story of vast migrations, of local ecotypical and geographical variation, of intermingling strains and replacements. The evidence that *G. arboreum* was the Old World species which reached South America, and that it did so directly from India, without a temporary stay in Indo-Malaya or Polynesia, is of extraordinary interest because of the light it throws on the origins of the ancient South American Indian civilizations.

Part Four is entitled The Significance of 'Gossypium' in Evolutionary Studies (Hutchinson and Stephens). Here the development of new characters is related to the significance of polyploidy in evolution. A radical conclusion is reached, that "among crop plants polyploids possess the characteristics of high variability, extensive differentiation from their wild prototypes, and great range and adaptability, that they are usually associated with evolutionary success, and it is here suggested that far from being variations of no long-term significance, they lie on a main line of evolutionary advance . . . the possibilities of variation in a polyploid are no less than in a diploid, and . . . much of the variability arising by mutation will be subject to selection from the beginning. The general tendency throughout is towards a restoration of the diploid condition." The reason why polyploidy is commonly observed to be associated with species differentiation but not with the establishment of major evolutionary lines is attributed to the fact that there has been insufficient time since the two great modern changes in the environment have intervened, namely, the extermination

and recolonization associated with the Ice Age, and the great changes in vegetation due to the spread of cultivation. A brief chapter on The Nature of Genetic Variability and the Origin of Discontinuity is followed by the final discussion of The Evolutionary Prospects of a Crop Plant. Here the immense influence of man as an environmental factor in modifying the sizes of populations, in altering the rigor of selection or changing its direction, and in modifying isolation is given major emphasis. "Adaptation to meet the conditions of a man-made environment may now be regarded as the major line of evolutionary advance..." "An even closer relationship exists between the crop plants and man himself. Here, there is a true symbiosis..." This explains the close relationship, noted by Vavilov, between the centers of origin of cultivated plants and the centers of development of human civilization. The evolution of cotton was first a part of the growth of the rice civilization; and in its transfer to South America, other crop plants, such as *Solanum*, *Cucurbita*, *Amarantus*, and *Canavalia*, and in particular *Phaseolus* beans went along. All of these but *Cucurbita* have centers of origin in India. The authors conclude at this point, without calling attention to the fact that if they are correct, Columbus discovered the way to India after all!

BENTLEY GLASS

EXPERIMENTAL STUDIES ON THE NATURE OF SPECIES. *III. Environmental Responses of Climatic Races of Achillea. Carnegie Institution of Washington Publication 581.*

By Jens Clausen, David D. Keck, and William M. Hiesey. Carnegie Institution of Washington, Washington, Washington, D. C. $3.00 (cloth); $2.50 (paper). iv + 129 pp.; ill. 1948.

Beginning with the genetically acceptable thesis that widespread species must be able to cope with a variety of ecological habitats, the authors have attempted, in this third of their series of monographs on experimental taxonomy, to explore the relationships between plants, specifically of the genus *Achillea*, and their environment in an effort to gain a better insight into the evolutionary processes that determine species differences. Employing their standardized transplant technique, and supplementing this with culture under controlled conditions, they have shown that climatic races are definitely fitted best for the habitat in which they are naturally found, this fitness relating both to form and to developmental processes.

The important conclusions may be summarized as follows:

1. Ecological races approach an equilibrium with their environment, this equilibrium being a balance of inherent genetic variation against environmental selection.

2. The equilibrium is consequently dynamic, and the ecological race shifts with the environment.

3. The races differ genetically among themselves, and this inherent variance permits a species to spread and new races to be established in different habitats.

4. Climatic races have as "their most important characteristic the ability to synchronize their activities with the diurnal and seasonal periodicities of their environments."

5. Race fitness to environment is a composite characteristic which is only an approximation, and which depends upon the fitness of individual members.

6. The migration and exchange of genes provide the materials for new biotypes.

These are conclusions which could have been stated empirically prior to this study, but their substantiation through experiment is an important step in the expanding field of experimental evolution. Important, too, is the fact that through a comparative study of the physiology of races, a more detailed examination of the processes of adaptation is rendered possible.

C. P. SWANSON

THE BEGINNING OF THE AGE OF MAMMALS IN SOUTH AMERICA. *Part I. Introduction. Systematics: Marsupialia, Edentata, Condylarthra, Litopterna and Notioprogonia. Bulletin of the American Museum of Natural History, Volume 91, Article 1.*

By George Gaylord Simpson. American Museum of Natural History, New York. $3.50 (paper). Pp. 1–232 + 19 plates; text ill. 1948.

This monograph is primarily concerned with a description of the three oldest of the known mammalian faunas of South America, the Riochican, the Casamayoran, and the Mustersan stages of Patagonia. Stratigraphic data are given, and the general stratigraphic problems are discussed. Classification and relationships of the animals and their bearing on the taxonomy and phylogeny of later South American mammals are dealt with in detail.

According to Simpson, "It would be impossible to overemphasize the importance of these faunas for an understanding of the history of life, particularly in South America but also in general. As far as such a key is yet available, they are the key to the origin of South American mammals, and they lead into the richest and most characteristic development of these. They also illustrate in the most interesting way and on the most magnificent scale the deployment and evolution of a fauna developing in isolation but on a continental scale. They display an experiment in evolutionary principles conducted for us by nature in a space and over a period of time far greater than any laboratory research can encompass."

The uncovering of these important faunas was due above all to the remarkable labors of the brothers

Ameghino. Simpson devotes a special section to their work, which he treats with great sympathy and understanding. "Any work on Patagonian fossils must in a sense be a monument to Florentino and Carlos Ameghino." When they embarked on their amazing career, Florentino was "an obscure provincial teacher, without formal scientific training or higher education"; and Carlos was "his almost unlettered [younger] brother" who, unfortunately, "kept almost no field notes" but who "kept most of his records in his head," so that they have been largely lost to science. Their Patagonian collaboration, begun in 1887, lasted 16 years. All of their collecting was undertaken at their own expense, and they were in constant financial difficulties. Yet, "between them, the older brother in the study and the younger in the field, they revolutionized South American geology and rewrote one of the most important chapters in the history of the earth."

Florentino exhibited a strange mixture of scientific strength and weakness. "The ambivalent mixture of reverence and distrust with which he was regarded in his life and which still affects his reputation is rooted in a deep-seated duality in his character, a complex antithesis that runs through all his work." To Simpson, there are three outstanding features in his work: "keenness and accuracy of morphological observation, breadth and insight in generalizing on the basis of the resemblances and differences detected, and a remarkable sense of sequence and ability for historical inference." On the other hand, "his detection of minute morphological differences was translated into an inflated taxonomy, his study of broad resemblances led to a strange dual phylogenetic scheme, half right and half wrong, and his sense of sequence was betrayed by a basic, subconscious fallacy into consistent bias as to the age of each member of the sequence." His failure to take individual variation into account, and his neglect of the phenomenon of evolutionary convergence or parallelism, were especially unfortunate, and led him to strange taxonomic and phylogenetic conclusions. His great egocentricity and nationalism led him to regard Argentina as the center of mammalian origins, and himself as the discoverer of the ancestral types. This contributed to his rather weird phylogenies. But, when all is considered, as Simpson points out, he must be regarded as a truly great man. "Insight into . . . Ameghino's personality and life serves only to strengthen sympathy for him and admiration for his achievements."

Carlos Ameghino was of a different mold, and his contribution "is probably less appreciated and understood, even today . . . 'Ameghino' has come to mean Florentino, but I think that the ultimate judgment of history may rank Carlos as high. He was certainly a genius as a field geologist and collector."

The reviewer, for one, feels grateful to Simpson not only for his scholarly revision of these early mammalian faunas, but also for his illuminating account of the life and work of the much misunderstood Ameghino brothers.

WILLIAM L. STRAUS, JR.

HALLMARKS OF MANKIND.

By Frederic Wood Jones. The Williams & Wilkins Company, Baltimore. $2.50. vi + 86 pp.; ill. 1948.

The author, a professor of anatomy in the venerable Royal College of Surgeons of England, has fathered this little book of which he says himself that it is "an apparent impertinence." This he specifies in his introduction with the blunt claim that "the familiar story of Man's origin as it is given in Darwin's *Descent of Man* and Huxley's *Man's Place in Nature* would be nearer to the truth if it were read backwards." Going backwards, the author repeats without many significant additions, but with some timely omissions, his thesis, reiterated for the past thirty years, that "Man is an extremely primitive type, and . . . though more primitive in basal structure than the living monkeys and apes, Man has his own remarkable structural specialisations that distinguish him from all other Mammals and appear to be his very ancient hallmarks."

Professor Jones' historical remarks, technical comments, and lengthy arguments are cleverly written and stimulating, but remain far from convincing. The author possesses great talent for magnifying with ardent verbosity those isolated details which tend to justify his sweeping conclusions and for belittling or, mostly, ignoring all evidence unfavorable to his views. By dwelling at length on a few strictly localized and highly variable cranial features, but with at most brief and vague consideration of other morphological conditions, the author admits having been "driven to the inescapable conclusion that Man could not possibly have been derived from any form that possesses the assemblage of anatomical characters typical of any Old World Monkey or Anthropoid Ape nor from any extinct form that possessed these definite catarrhine specialisations." The newly discovered South African fossil apes are labelled "spurious claimants to human ancestral status."

Disproportionate space is given to a technical discussion of one detail in the development of the premaxilla in gorilla and in man. Certain adaptations in the human foot have also been singled out for special description to lend some factual support to the author's lavish theories and arguments. Here it can merely be mentioned that some of the facts presented are not in agreement with observations by other authors. Facts, however, form only a background for this challenging, imaginative, and unorthodox interpretation of man's part in primate evolution.

The booklet ends with the confident prediction: "If the Primate forms immediately ancestral to the human

stock are ever to be revealed, they will be utterly unlike the slouching, hairy, 'ape men' of which some have dreamed and of which they have made casts and pictures during their waking hours; and they will be found in geological strata antedating the heyday of the great apes."

A. H. SCHULTZ

STRANGE PREHISTORIC ANIMALS AND THEIR STORIES. *By A. Hyatt Verrill. L. C. Page & Company, Boston.* $3.75. xvi + 262 pp. + 2 plates; text ill. 1948.

Verrill writes entertainingly, and he has done a number of good books, but this one is not among them. He excuses himself, in the Preface, for not paying attention to the chronological sequences of the prehistoric animals, and this may be allowed, considering his audience of young people. But it is not equally allowable to mix indiscriminately the animals of different times and portray them as contemporaries. Over and above this, however, the book is so full of errors that it would be no safe guide to any young biologist. *Ichthyosaurus* is made out to be huge as a whale, and the marine turtles are said to be more closely related to the plesiosaurs than to the "true" turtles and tortoises. The rhinoceroses are considered to be possibly descendants of the ceratopsian dinosaurs, and *Sphenodon* is related closely to the iguanas. The average number of such errors is about one to the page.

Nature and Mother Nature are personified, as in such sentences as the following: "Perhaps Nature, not being quite certain which form of mammal would be most successful in the long run, created these combination beasts with the idea that those features best adapted to survival would develop and predominate, while the others would be eliminated" (p. 126). Lamarckianism is openly accepted. The poor aardvark "spends his days hidden from sight in subterranean burrows as though thoroughly ashamed of his nudity and freak make-up" (p. 174). In short, what passes for fact in this book is hardly distinguishable from the frank fiction of the fabulous creatures of ancient times, the dragon, the lamia, and the manticore. There is an interesting chapter on sea serpents. The final chapter is on ancient mythology, Greek, Roman, and Norse, then describes the medieval vampires and were-wolves, and ends up with Paul Bunyan and his blue ox, Babe.

BENTLEY GLASS

GENETICS AND CYTOLOGY

GENETICS IS EASY. *A Handbook of Information for Teachers and Pupils.*

By Philip Goldstein. Garlan Publications, Hoboken. 75 cents (paper). 72 pp.; ill. 1947.

This little book represents a laudable effort to make a condensed yet simple version of the principles of genetics. None have been slighted, from the well-known Mendelian laws to the newer findings of biochemical genetics. Some mistakes have been made, particularly in regard to synapsis and crossing over, which are described as 2-strand rather than 4-strand processes. But the merits of the exposition far outweigh its minor defects, and any teacher of genetics will find in the diagrams and discussions numerous aids to clear presentation.

Perhaps the most noteworthy thing about the book is its author's modesty. He has had the courage—and wisdom—to insert a tear-sheet at the end of the book, with space for notations of errors, difficulties, omissions, and suggestions, to be returned to the author. Many a weightier book has more need for such a thing.

BENTLEY GLASS

ACTA GENETICA ET STATISTICA MEDIA. *Volume I, Number 1, 1948.*

Edited by Gunnar Dahlberg; in association with Otto L. Mohr and Tage Kemp. S. Karger, Basel and New York. Sw. frs. 44.—, per volume. Published quarterly.

Gunnar Dahlberg, the editor of this new journal of human genetics, points out in an Introduction that there is more need of attention to the question of the future make-up of populations for which data on their present composition and on the crossings within them are available. Although the older problems of familial genetics will not be neglected, it is to the genetics of populations that this journal is primarily to be devoted. Investigations of a statistical nature are particularly appropriate for it.

The first issue contains four papers: The Prognosis in Schizophrenia (S. Stenberg); The Eruption of the Permanent Teeth in the Normal Population of Sweden (G. Dahlberg and A. B. Maunsbach); The Angle of the External Ear to the Median Plane of the Skull in Children (L. Linder); Environment, Inheritance, and Random Variation with Special Reference to Investigations on Twins (G. Dahlberg).

DIE VIERLINGSGESCHWISTER GEHRI UND IHR VERWANDTSCHAFTSKREIS. *Eine Familienanthropologische Untersuchung. Archiv der Julius Klaus-Stiftung, Band XV, Heft 3/4.*

By Otto Schlaginhaufen. Art. Institut Orell Füssli, Zürich, Switzerland. Paper. Pp. 309–398 + 7 plates + 11 charts. 1940.

The author presents the results of detailed anthropometric and dermatoglyphic studies on a set of quad-

ruplets, consisting of two males and two females. It is concluded, on the basis of numerous morphological dissimilarities, that they originated from four separate zygotes. Even with allowance for the fact that the paper was published in 1940, the reviewer was impressed by the author's failure to utilize serological data in arriving at his decision (A-B-O, M-N, secretor non-secretor). Two half and two full siblings of the quadruplets were also studied. The resemblances among the quadruplets are greater than those of the quadruplets to their full (or half) siblings, a fact attributed to the greater similarity of the environmental factors operating on the quadruplets. Extensive geneological studies on the ascendants and collaterals of the quadruplets revealed a significant increase in the frequency of multiple births in several different lines.

JAMES V. NEEL

THE HYBRID-CORN MAKERS: *Prophets of Plenty.* *By A. Richard Crabb. Rutgers University Press, New Brunswick.* $3.00. xxvi + 331 pp. + 16 plates. 1947.

From both the economic and biological points of view, hybrid corn is a development of great significance. This significance has been partially stated in *The Hybrid-Corn Makers.* The first seven chapters of Richard Crabb's book constitute an interesting, accurate, well-written account of the early history of corn inbreeding and hybridization. In this section of the book the author has obviously gone to considerable length through personal interviews and correspondence to establish the details of motivation and design of experiments and the personal backgrounds and views of the pioneer corn breeders. For the reviewer the following facts and evaluations presented in these chapters were of particular interest:

(1) The corn-breeding program initiated at the beginning of the century by Cyril G. Hopkins at the University of Illinois was a direct outgrowth of a meeting held in March, 1896, to discuss the deficiencies of corn as a diet for livestock. The immediate aim of Hopkins' breeding program became to produce strains of corn with a higher protein content.

(2) It was in connection with the chemical analyses on Hopkins' various lines of maize that Edward Murray East first entered the corn work. Hopkins needed a chemist and hired East in June, 1900, on the basis of his outstanding record as an undergraduate student in chemistry at the University of Illinois.

(3) East's move from Illinois to the Connecticut Experiment Station was apparently conditioned by two main factors: a personal conflict with Hopkins in 1905 regarding the significance of inbreeding experiments and a timely, attractive offer the same year by E. H. Jenkins, Director of the Conn. Exp. Sta. With Jenkins' offer of a position on the scientific staff of the Station went a promise of complete freedom in planning the corn-breeding work. Thus, as a result of Jenkins' insight and judgment, East became established at Connecticut, and under East's direction and influence, there developed that long-range program of experimental inbreeding and crossbreeding which ultimately make hybrid corn a practical reality.

In this section of the book, Crabb gives East the credit he deserves and states clearly that the contribution made by George H. Shull to hybrid corn was that of its *theoretical possibility.* From the beginning, East recognized the difficulties inherent in Shull's "pure-line method of corn breeding" (i.e., hybridization between two inbreds), and these difficulties were not wholly removed until Jones developed the double cross hybrid at the Connecticut Station.

With Jones' development of the double cross, in 1920, the history of hybrid corn as a scientific achievement comes to an end for the author. The rest of his book—twelve chapters, comprising 226 of the total 318 pages—is devoted to an account of the campaign waged in "selling" hybrid corn to the farmers. This "salesmanship," of course, consisted primarily in the perfectly tremendous task of producing corn hybrids suitable to various localities and conditions. For this invaluable contribution, author Crabb awards top honors to the commercial seedsters—particularly to J. R. Holbert and the Funk Bros. Seed Co. He finally goes to the utter extreme of saying that "no person has done more to give the present generation of Americans hybrid corn than Jim Holbert." F. D. Richey (J. Hered., 39: 15, 1948) has taken particular exception to this exaggerated praise. Himself an authority in corn breeding, Richey states: "The Companies have produced no hybrids demonstrably superior to those of the State and Federal breeders. And this in spite of the fact they had full access to the State and Federal inbreds as well as their own."

It is certainly regrettable that Crabb has devoted such a large portion of his book to the repetitious and tiresome praise of the commercial seed producers. It is also regrettable that before writing his book he did not make a more thorough study of both the early and current views regarding the genetic nature of heterosis. Throughout the book one has the feeling that the author's comprehension in this matter begins and ends with Shull's original statement.

HELEN CROUSE

ESSENTIALS OF GENERAL CYTOLOGY. *Edinburgh University Publications, Science and Mathematics Number 2.* *By R. A. R. Greeson, including four chapters on Plant Cytology by Helena Heslop Clark. Edinburgh, at the*

University Press. 21s. x + 184 pp. + 17 plates; text ill. 1948.

This text presents cytology in a most elementary fashion, being designed to meet the needs of a short course preliminary to a study of genetics, or as a separate course complete in itself. As a consequence, cytogenetics is but briefly mentioned, with greater emphasis being placed upon reproduction and cytoplasmic structures and functions. The latter subjects are very well handled, and the author should be commended for his excellent illustrations. Two major criticisms, however, can be made of the book in general. The first is that the brevity of treatment has forced the author into a reportorial style which gives an air of finality to many statements about which there is still no good agreement among cytologists. An instructor using the textbook would necessarily be forced to fill in many of the obvious gaps. Secondly, the author accepts wholeheartedly the Darlingtonian hypotheses regarding mitosis, meiosis, chromosome chemistry, and cytoplasmic inheritance. This can only result in a biased presentation of some of the most important aspects of present-day cytology.

With an expanded treatment and a less biased point of view, this could have been an excellent book. In its present form it is not likely to present a challenge to an inquiring student.

C. P. SWANSON

THE CYTOPLASM OF THE PLANT CELL. *A New Series of Plant Science Books, Volume VI.*

By Alexandre Guilliermond; authorized translation by Lenette Rogers Atkinson. Foreword by William Seifriz. Chronica Botanica Company, Waltham, Massachusetts; Stechert-Hafner, New York. $5.00. x + 247 pp.; text ill. 1941.

Although this book was published in 1941, war-time restrictions on publication permitted the issuance of but a limited number of copies. A recent second printing once more makes available to the cytologist and physiologist this excellent book on cellular structures exclusive of the nucleus. Although the treatment concerns itself mostly with plant materials, it is not without value to the zoologist.

The book deals principally with the cytoplasm, chrondriosomes, plastids, and vacuoles. The theories dealing with the structure and function of these systems are many and conflicting; yet the late Dr. Guilliermond, who has contributed greatly in this field through his own researches, has presented them in a clear and unbiased manner. He leaves the reader in no doubt as to his own interpretations; but, on the other hand, the data and interpretations of other workers are given sufficient treatment to allow the reader to form his own opinions. Where present information is insufficient for constructive theorizing, the author has judiciously refrained from unnecessary speculation. In a discussion of the function of the chrondriosomes, for example, he states that "nothing is positively known about the role of the chrondriosomes." Such a refreshingly frank statement concerning a subject about which he was a recognized authority is characteristic of the book; yet, rather than conveying an attitude of defeatism, the book serves to point out where further information is needed, clearing the way, as it were, for a more concentrated effort on those problems which require clarification. Scientist that he was, Guilliermond has not separated structure and function; and the book therefore presents a more integrated point of view of the cellular organelles than has been previously published. He does not, however, carry the idea to its logical conclusion, as does Frey-Wyssling. The latter conceives of the cytoplasm as a lattice-work of interlacing submicroscopic particles, stating that there can be no specificity of function without a similar specificity of structure. Guilliermond has followed the older ideas in considering the cytoplasm to be simply a colloidal system with various proteins in solution.

C. P. SWANSON

GENERAL AND SYSTEMATIC BOTANY

GENERA FILICUM, *the Genera of Ferns. Annales Cryptogamici et Phytopathologici, Volume V.*

By Edwin Bingham Copeland. Chronica Botanica Company, Waltham, Massachusetts; Stechert-Hafner, New York. $6.00. xvi + 247 pp. + 10 plates; text ill. 1947.

Copeland presents in *Genera Filicum* the results of a lifetime study of the ferns of the world. He has had unusually wide opportunity for both field and herbarium investigations, and the wisdom to bring to his taxonomic studies broad interests in the biological world. The book will be indispensable to all students of the ferns.

The author gives two reasons for writing and publishing the book. First, progress in the taxonomy of the ferns during the past fifty years has been so great that no existing general work now does justice to the field. Second, he feels that after many years of work and the publication of many papers dealing only with parts of his subject, he has an obligation to publish a work integrating and summarizing the whole, relating it to the studies of predecessors and contemporaries. A brief introduction, of about ten pages, contains an elaboration of these two purposes. First is a discussion of the history of pteridology and of the developing concepts of species, genus, and family as they are applied to the ferns. Linnaeus listed only 14 genera in the group, whereas in this book 305 are treated. With regard to the second purpose of the book, the author

gives an all-too-brief account of his own development as a student of ferns.

The main body of the book is descriptive of the known orders, families, and genera. Keys are given for the genera within each family, and to the families within each order. Generic synonymy is complete, and under each genus is given its type species, range, and a discussion of literature especially pertinent to it. There are ten plates of illustrations and a comprehensive index of names. The book is handsomely printed and bound, with well-chosen vignettes at suitable places, in the usual manner of Chronica Botanica publications.

H. M. RAUP

AZALEAS: KINDS AND CULTURE.

By Harold Hume. The Macmillan Company, New York. $3.75. x + 199 pp. + 7 plates; text ill. 1948.

The eastern and southeastern United States have long been aware of the horticultural advantages of both deciduous and evergreen azaleas. This is not true of other parts of the country. Yet, as the author points out, these most desirable of shrubs have such a variety of cultural requirements, hardiness, and flowering time as to be adapted to almost any portion of the country except the drought areas. Not all species are sufficiently hardy to grow well above the Mason-Dixon line, but among the Asiatic and American species are many that can withstand severe winters. This book should do much to dispel the common notion that azaleas are difficult shrubs to grow, and that their period of bloom is too short to warrant giving them garden space. It will probably be of some surprise to many to learn that, in the South, and by the proper selection of species, azaleas can be had in bloom from September to July. The season, however, will obviously be shortened as one progresses northward.

The author adequately discusses cultural and propagation methods, and provides detailed tables of the varieties and species adaptable to various climatic conditions. Well illustrated in both color and black-and-white, the book makes fascinating reading for the garden lover and furnishes, as well, a sound basis for the selection and care of those azaleas most suitable for a particular area. It should find a favored place in the gardener's library.

C. P. SWANSON

FACTORS IN BOTANICAL PUBLICATIONS AND OTHER ESSAYS. *Chronica Botanica, Volume 11, Number 3. By Neil E. Stevens. Chronica Botanica, Waltham, Massachusetts; Stechert-Hafner, New York.* $2.00 (paper). Pp. 119-206. 1947.

Scientific essays, short or long, formal or informal, often bring to light a facet of the author's character and mind which is never, or but seldom, revealed in his more matter-of-fact publications in scientific journals. As a consequence, such essays serve as overflow valves through which issue the ideas, musings, generalities, and philosophies which find no logical inclusion in strictly research papers. In such form they may add to the philosophical background of a science in an instructive or a broadening manner, they may serve to give a more integrated point of view of a subject to both scientist and layman, or they may simply provide entertainment for the reader—and this last is mentioned in no unworthy sense. The author of the present essays is a prolific writer; his nearly 200 publications are sufficient proof. As to his eminence in the field of plant pathology there can be no question. One can, however, question the editor's statement that these essays are of "international importance" and "unusual permanent interest." Concerned largely with botanical trivia such as the botanical allusions in the writings of the New England poets and in the Bible, the meekness of American botanists, and fads among research botanists, these essays cannot lay claim to any great enrichment of American botany, as Wheeler's essays have done for entomology, or East's for genetics. Stevens obviously must have enjoyed writing these essays; one can only criticize the publishers for having resurrected them.

C. P. SWANSON

ECONOMIC BOTANY

CHEMICAL FUNGICIDES AND PLANT INSECTICIDES. *A Catalogue of Insecticides and Fungicides, Volume II. Annales Cryptogamici et Phytopathologici, Volume VIII. Compiled by Donald E. H. Frear; with a foreword by F. F. Lininger. Chronica Botanica Company, Waltham, Mass.; Stechert-Hafner, New York.* $5.50 (paper). xii + 154 pp.; text ill. 1948.

Volume I of this catalogue was reviewed in QRB 23: 361, 1948. In volume II the coding system of compounds, necessary to the proper understanding and use of the catalogue is repeated. Whereas the previous work dealt solely with chemical insecticides, this one lists chemical fungicides, condensation products, plant product fungicides (listed alphabetically under generic names), miscellaneous fungicides, plant insecticides (also listed alphabetically by genus), and miscellaneous plant products. As before, there is a combined reference and author index and a numerical patent list. An index of all chemical compounds listed in both volumes is included here. In such a complete work, it is perhaps

unfortunate that an index of insect species was not also included. With the help of the other indices which are present it is possible to locate any compound or plant product with a minimum of difficulty. Under each entry are to be found its uses and references to published reports. This tremendous compilation will indeed spare workers in entomology, plant pathology, and chemistry the necessity of long and tedious searches of the literature.

V. G. DETHIER

GENERAL AND SYSTEMATIC ZOOLOGY

ZOOLOGY: *The Science of Animal Life.*

By A. M. Winchester. D. Van Nostrand Company, New York, Toronto, and London. $4.50. xii + 437 pp.; ill. 1947.

The aim of this new textbook is to offer a survey course covering the main principles of zoology, with particular emphasis on those phases of the subject closely related to man and his everyday life. It is to be expected that students who wish to go further into the field can more efficiently obtain the needed details in advanced courses. Technicalities are considered of little general educational value and only dampen the student's interest in the subject. With this view in mind, scientific jargon has been pared to a minimum, and the vocabulary which is considered essential is built up slowly and carefully defined.

This is an admirable point of view, and the author has been highly successful for the most part in his presentation. There is bound to be diversity of opinion regarding proper emphasis. Whereas the animal kingdom appears to be rather completely surveyed, it seems strange that the rat-like rodents are dismissed in two sentences, while the civet cats receive three, and the sloths, four. The survey from protozoa to mammals requires 330 pages, yet the principles of zoology, such as the life processes, heredity, evolution, etc., are described in 86 pages. We believe that a shift in emphasis here would make this a much more useful text. Simplification always leads to misleading statements such as: "tiger salamander . . . *seems to like* its larval existence; pigeons secrete a liquid from the *stomach*; arrival of the swallows at the mission San Juan Capistrano in California occurs on *exactly* the same day *each* year" (italics added). The text is profusely illustrated, many of the photographs having been taken by the author himself. Index and glossary are provided.

HENRI C. SEIBERT

ZOOLOGY LABORATORY STUDIES (INVERTEBRATE AND VERTEBRATE).

By Guy P. Thompson; foreword by Norman E. Phillips. The C. V. Mosby Company, St. Louis. $4.25 (paper). 361 pp.; ill. 1948.

The amount of material in this manual is more than usual in most laboratory workbooks. It is designed for use in those zoology courses which extend throughout the year. By selecting various exercises it can also be used for briefer courses of one semester. The section on vertebrates has sufficient material on the amphioxus, shark, frog, and cat to be used in a comparative anatomy course. For each invertebrate described, there is a definition of its phylum, its classification, its habitat and size, its external features, directions for dissection, and a list of labels to be included on the drawings. A similar plan is used for the vertebrates, except that internal anatomy is studied with greater detail. The descriptive material is almost sufficient to enable this manual to be used as an auxiliary textbook. There are charts and tables of muscle origins and insertions, of cranial nerves, of bones of the skeleton. Original illustrations help the student in the more difficult dissections. Important topics in comparative anatomy, such as the fate of the aortic arches, the homologies of the urogenital system, and the origin of the middle ear are also illustrated. Sections on handling the microscope, on making laboratory drawings, and on mitosis, meiosis, histology, and embryology are to be found in appropriate places in the text.

It appears that this is a most thorough book—in fact, much too thorough for any one-semester course in general zoology, even were most of the exercises to be eliminated. But for longer or more advanced courses it should prove useful. For the most part B.N.A. terms are used for the circulatory system. The terminology of the carpals and tarsals is based on human anatomy rather than that employed by comparative anatomists. One erroneous statement was noted: the description of mitosis in both figure and text indicates that the spireme divides into segments to form chromosomes.

HENRI C. SEIBERT

THE LAND AND FRESH-WATER MOLLUSKS OF PUERTO RICO. *Miscellaneous Publications, Museum of Zoology, University of Michigan, Number 70.*

By Henry van der Schalie. University of Michigan Press, Ann Arbor. $2.50 (paper). 134 pp. + 15 plates; text ill. 1948.

Puerto Rico occupies an interesting zoogeographic position. It is the most remote of the Greater Antilles, yet it lies in greater proximity to some of the Lesser Antilles than to the nearest neighbor in its own group. Its terrestrial fauna, while rich, lacks the highly colored molluscan genera *Liguus* and *Polymita* of Haiti and Cuba, and most of the diversified land operculates of Jamaica. Nor do the Strophocheilidae of South

America, which have invaded the Windward Islands, extend to Puerto Rico. On the other hand, the genus *Gaeotis* is peculiar to the island, and there are several peculiar species, among them one each of *Austroselenites*, *Gulella*, and *Nenia*. The last two are surprising, for *Gulella* is an African genus, and *Nenia* is Peruvian.

From the standpoint of zoology, then, the island has considerable individuality. It is a natural zoogeographic unit, and affords an excellent opportunity for the study of zoogeographic relations and of the way in which these have in turn been affected by physiography and geology. The author has gone carefully into these matters, as well as into the history of the zoological exploration of the island, so that his work is far more than a catalog of names, descriptions, maps and designations of localities, and bibliographic references.

Van der Schalie has introduced some rather radical taxonomic changes. Among these may be mentioned the consolidation of the Lymnaeidae and Planorbidae into a single family; the transference of the genera *Thysanophora*, *Mcleania*, and *Microconus* from the Sagdidae to the Polygyridae; of *Opeas* from the *Subulinidae* to the Férussaciidae; of *Caecilioides* from the Férussaciidae to the Systrophiidae; and the use of the name Cepolidae for the family typified by *Cepolis*. (Should this name not have more properly been Cepolitidae, rather than Cepolidae?)

There is a 4-page index of all taxonomic terms used in the book. Ten excellent plates of drawings, a portrait of F. M. Pagán, and four photographic plates of the Puerto Rican countryside illustrate the text.

JOSHUA L. BAILY, JR.

THE INSECT GUIDE. *Orders and Major Families of North American Insects.*

By Ralph B. Swain; Illustrated by SuZan N. Swain. Doubleday Company, Garden City, New York. $3.00. xlviii + 261 pp. + 56 plates; text ill. 1948.

It is inevitable that users of *The Insect Guide* will tend to compare it with similar handbooks, such as Lutz's *Field Book of Insects*, which have appeared in the past. This new volume, however, aspires to a different goal. The stated purpose is "... to present the insects of North America north of Mexico, in pictures and in nontechnical language, at the family rather than at the species level, and to describe as fully as space permits the 'niches' they fill in animal and plant communities." In this it succeeds. A short introductory section presents in word and picture the salient features which distinguish an insect from its near relatives. Brief reference is made to the relations between insects and other organisms, e.g., plants, animals, and man. A few pages which are devoted to structure, growth, and development follow. Directions for using the book to fullest advantage are then given.

The body of the guide is arranged by orders, from the Protura to the Siphonaptera. Within each order, the treatment is by families. For each family, a diagnostic description of the adult and immature stages is given. Then follows a statement concerning the economic importance of the group, and finally a description of one or more typical members. With each example there is a number which refers to an illustration. Both scientific and common names are given throughout. In the index, insects are listed under both names. Reference is to illustration and family number rather than to page. The system works conveniently in this volume.

The illustrations by Mrs. Swain are particularly noteworthy. Forty-eight plates in full color and numerous stippled figures illustrate more than 250 species. The stippled drawings are excellent, and the colored plates are the best that have appeared in any general entomological text in America. Reproduction seems to be a shade better in the colored plates of the *Nouvel Atlas d'Entomologie* (see p. 152 of this issue, QRB) than here.

Altogether, *The Insect Guide* is to be highly recommended as a beginner's handbook for budding young entomologists and for the interested lay public. It should prove of considerable value to gardeners also. It is indeed a stimulating little volume. It is unfortunate that it is not provided with a more serviceable cover and binding.

V. G. DETHIER

THE SONGS OF INSECTS. *With Related Material on the Production, Propagation, Detection, and Measurement of Sonic and Supersonic Vibrations.*

By George W. Pierce. Harvard University Press, Cambridge. $5.00. viii + 329 pp. + 1 plate; text ill. 1948.

A great variety of sound-making animals have been examined with precise physical instruments in Pierce's laboratory at Harvard, and under more nearly field conditions each summer at his residence in Franklin, N. H. In this impressive book, Pierce sums up the results and presents clear photomicrographs of the sound-producing structures, photographs of the insects involved, sample records of their call notes, and a careful analysis of each. Twelve kinds of crickets, 18 different long-horned grasshoppers and katydids, 8 species of locusts, and 2 of cicadas are so considered, and 4 birds and 2 bats are thrown in for good measure. In proper Harvard tradition, all the insects analysed are males. A few female katydids can and do stridulate, but no mention is made of this fact. However, sex does enter the picture for the familiar black cricket, which chirps along in a highly audible key until a mate appears. Then the note rises from 4900 vibrations per second to 17,000—above the limit of human ears.

Some of the long-horned grasshoppers achieve 40,000 cycles of supersonic activity, but the notes we hear are more commonly in the range around 8000. Bats, on the other hand, squeak at 7500 and use 48,000 for sonar location of obstacles.

Each of the insects discussed is treated under its scientific name, which will make the volume of great help to entomologists. They may be dismayed to find that the small cricket *Nemobius fasciatus* can be identified to subspecies easily with a sonic analyser, but that no simple morphological characters allow such separation! And any temptation to duplicate Pierce's records may run into difficulties. Considerable space is given over to a description of the apparatus and the methods of use, with an appendix on the performance characteristics of the equipment. But at several points the wiring diagrams are sufficiently vague (regarding coupling transformers, anti-resonant circuit, etc.) that without further information it would not be easy to construct a sonic amplifier such as that which the author has used so successfully.

The book is primarily a report on the Harvard studies. The few bibliographical references to other workers, to technical discussions of some components of the electronic equipment, and to various patents, are included in footnotes. There is no index.

LORUS J. MILNE & MARGERY J. MILNE

EXPEDITION TO SOUTH-WEST ARABIA, 1937–8. *Volume I, Numbers 9–15.*

British Museum (Natural History), London. 12s. (paper). Pp. 67–178 + 6 plates; text ill. 1948.

This latest group of papers dealing with the taxonomy of insects collected by the Arabian expedition ten years ago consists of treatments of the following orders and families: Trichoptera, Coleoptera (Cicindelidae, Carabidae, Dryopidae, Elmidae, Scarabaeidae, Corylophidae), and Diptera (Muscidae). Nine interesting photographs of the country through which the expedition passed are included.

V. G. DETHIER

ATLAS DES LIBELLULES DE FRANCE. *Belgique, Suisse, Number 3.*

By L. Chopard; illustrations by Germaine Boca. Éditions N. Boubée & Cie., Paris. 250 fr. (paper). 137 pp. + 12 plates; text ill. 1948.

ATLAS DES LÉPIDOPTÈRES DE FRANCE. *Belgique, Suisse, Italie du Nord. II. Hétérocères, Number 6. By Claude Herbulot; illustrations by R. Méaye and A. Moreau. Éditions N. Boubée et Cie., Paris.* 350 fr. (paper). 145 pp. + 16 plates; text ill. 1948.

ATLAS DES HÉMIPTÈRES DE FRANCE. *I. Hétéroptères, Gymnocérates, Number 4.*

By A. Villiers. Éditions N. Boubée & Cie., Paris. 210 fr. (paper). 83 pp. + 12 plates; text ill. 1945.

ATLAS DES HÉMIPTÈRES DE FRANCE. *II. Hétéroptères Cryptocérates, Homoptères, Thysanoptères, Number 4.*

By A. Villiers. Éditions N. Boubée & Cie., Paris. 210 fr. (paper). 113 pp. + 12 plates; text ill. 1947.

Four more volumes have been added to the *Nouvel Atlas d'Entomologie* (other volumes reviewed in QRB. 23: 77, 251, 1948). The same high standard of workmanship is apparent in these presentations of the moths, dragonflies, and Hemiptera. Although these volumes, like some of their predecessors, are primarily taxonomic, each contains introductory remarks of general interest. The illustrations are superb.

V. G. DETHIER

THE BIRDS OF KOREA. *Bulletin of the Museum of Comparative Zoology at Harvard College, Volume 101, Number 1.*

By Oliver L. Austin, Jr. Museum of Comparative Zoology at Harvard College, Cambridge. $3.00 (paper). Pp. 1–301 + 1 plate. 1948.

From 1906 to 1945 the Japanese ran Korea much in their own way, ornithologically as well as politically. As a result, occidentals have not had any great opportunity to study the Korean bird fauna, especially since specimens from this area are relatively scarce in most collections. This volume therefore promises to be an invaluable source of information. The author made use of his Service stay in Korea to collect specimens and to investigate local bird collections. After his discharge he returned to Japan, where he made further studies of Japanese collections and had opportunity to discuss his problems with Japanese ornithologists. It is to be regretted that many of the collections, both in Korea and Japan, have been scattered or destroyed as a result of the war.

This list includes 355 species of birds known to occur, or to have occurred, in Korea. Only those species are accepted for which at least one specimen has been collected. The distribution of each species is recorded according to each of the 13 provinces, since a more definite geographical allocation was found to be impossible because of the confusion and instability of place names. The date of collection and the place where the specimen is (or was) deposited are included. There are additional notes on migration, breeding, and general status of the species on the peninsula. English and Japanese common names are provided.

This information alone would make this a worthy publication, but equally valuable are 1) a historical sketch which details the little known ornithological history of Korea and 2) a most complete bibliography,

which provides references to a large number of rare and unavailable Japanese publications. The general context of each article is briefly described.

HENRI C. SEIBERT

ECONOMIC ZOOLOGY

COMMON MARINE FISHES OF CALIFORNIA. *Fish Bulletin Number 68.*

By Phil M. Roedel. Bureau of Marine Fisheries, Division of Fish and Game, Department of Natural Resources, State of California. Paper. 150 pp. + 1 plate; text ill. 1948.

AGE AND LENGTH COMPOSITION OF THE SARDINE CATCH OFF THE PACIFIC COAST OF THE UNITED STATES AND CANADA, 1941–42 THROUGH 1946–47. *Fish Bulletin Number 69.*

By Frances E. Felin and Julius B. Phillips. Bureau of Marine Fisheries, Division of Fish and Game, Department of Natural Resources, State of California. Paper. 122 pp. 1948.

A PRELIMINARY POPULATION STUDY OF THE YELLOWFIN TUNA AND THE ALBACORE. *Fish Bulletin Number 70.*

By H. C. Godsil. Bureau of Marine Fisheries, Division of Fish and Game, Department of Natural Resources, State of California. Paper. 90 pp.; text ill. 1948.

The first of these bulletins is a revision of earlier bulletins of similar character, and is intended as a layman's handbook to the more common commercial and sport fishes of the California coast. The prefatory matter includes a glossary, anatomical diagrams, and a key. Each species is illustrated by a photograph, and information is provided on the relationships, distinguishing characters, distribution, fishing season, and type of fishing gear for each species. It is provided with a brief bibliography and an index. The other bulletins are composed principally of statistical data, preceded by brief analytical discussions. Preliminary evidence indicates that the California stock of yellowfin tuna is distinct from other Pacific stocks.

J. W. HEDGPETH

THE PRONGHORN ANTELOPE AND ITS MANAGEMENT.

By Arthur S. Einarsen. The Wildlife Management Institute, Washington, D. C. $4.00. xvi + 238 pp.; ill. 1948.

This book forms another addition to the series of monographs on the management and life histories of American game animals. While it represents work carried out largely on Oregon antelopes, its scope is such that it forms a general and highly authoritative treatise. The characteristics and life story of the antelope have been observed and recorded in a manner entirely in keeping with modern methods of research in natural history. These sections of the book are supplemented by many illustrations, largely taken from photographs.

Einarsen's style is generally good, and the interested reader will find his attention well rewarded. Biologists not familiar with the methods of game management will also be greatly interested in the techniques for maintaining antelope herds at a peak that permits a certain amount of hunting. The use of airplanes teamed with automobile patrol units is noted as the latest method of exerting an efficient control over hunters in an extremely wild area. This serves as a dramatic illustration of the value placed upon the recreational importance of game animals to our present society.

JOHN CUSHING

ANIMAL GROWTH AND DEVELOPMENT

GROWTH. *In Relation to Differentiation and Morphogenesis. Symposia of the Society for Experimental Biology, Number II.*

Academic Press, New York; Cambridge University Press, Cambridge, Eng. $7.50. viii + 365 pp. + 10 plates; text ill. 1948.

One of the contributors to the present volume, which incorporates most of the papers delivered at a Symposium of the Society for Experimental Biology held in Cambridge, England, in July, 1947, has begun his discussion with statements to the effect that "in a symposium of this type, a mere review of the literature or a simple presentation of one's own work seems inadequate. Such a meeting provides an opportunity and even an obligation on the part of the speaker to discuss the topic in general terms, and to present his own critical evaluation and summary of the literature and any working hypothesis he may have." The participants in this meeting have fulfilled this responsibility to a degree which is remarkably high. Many of the contributions, in fact, are of such high calibre as to deserve comment in their own right which space does not permit. It must therefore suffice principally to enumerate them.

Excellent modern discussions of various aspects of morphogenesis in plants are presented by F. G. Gregory, K. C. Hamner, R. Harder, E. Ball, Mary Snow and R. Snow, O. V. S. Heath and M. Holdsworth, and C. W. Wardlaw; and in addition, F. J. Richards has provided an interesting analysis of the geometry of phyllotaxis and its origin. On the side of animal biology, a paper by P. J. Gaillard on the explanation of endocrine glands, one by J. Z. Young on the growth and regeneration of nerve fibers, and one by S. Hörstadius and T. Gustavson present able discussions of special problems. A

communication by J. Holtfreter on concepts of the mechanism of embryonic induction and their relation to parthenogenesis and malignancy is especially notable for its warning that various experiments reported "leave little doubt that malignancy and lack of an organized development are quite different phenomena."

Other analyses of fundamental problems of general interest include a contribution on the role of the cell in determination by V. B. Wigglesworth, discussions of the actions of genes in development by C. H. Waddington, H. Grüneberg, and E. Hadorn, by K. Mather in his discussion of the nucleus and cytoplasm in differentiation, and by S. Spiegelman in his presentation of the plasmagene theory.

All biologists interested in growth will find stimulating the final essay embodying J. H. Woodger's Observations on the Present State of Embryology, and many will agree with his contention that in embryology "the labours of an army of ingenious experimenters have given us an enormous number of generalizations, many of them of a very complicated and puzzling nature, but, for lack of the right kind of hypotheses, we cannot yet be said to have mastered them." We clearly need, as Woodger has stated, "to devote some attention to questions of theoretical method." Whether, however, the resolution of our difficulties is feasible by making, as Woodger suggests, a deliberate and concerted attempt to derive key hypotheses from a few key data, is open to question. Harvey, Darwin, and Mendel approached their problems from the angle of interest in their materials, not from the point of approach of methodology as such. Clarification of hypotheses must await intelligent discussion of embryological data. In presenting such discussion in a book which deserves a place on the shelves of every professional embryologist, the Society for Experimental Biology has done much to promote the study of growth and development.

JANE OPPENHEIMER

ANIMAL MORPHOLOGY

ANATOMY, ANTHROPOLOGY, EMBRYOLOGY AND HISTOLOGY. *Being Section I of Excerpta Medica. A Complete Monthly Abstracting Service of Every Article From Every Available Medical Journal in the World Comprising 15 Sections and Covering the Whole Field of Clinical and Experimental Medicine. Volume I, Number 1. January 1947.*

Edited by M. W. Woerdeman, A. P. H. A. De Kleyn, and W. P. C. Zeeman. N. V. Excerpta Medica, Amsterdam. Sections I, IV, VIII, each $22.50 or £5.12s.—per yearly volume of about 900 pages.

Excerpta Medica is to be published in fifteen sections, the others being as follows: II, Physiology, Biochemistry, and Pharmacology; III, Endocrinology; IV, Medical Microbiology and Hygiene; V, General Pathology and Pathological Anatomy; VI, Internal Medicine; VII, Pediatrics; VIII, Neurology and Psychiatry; IX, Surgery; X, Obstetrics and Gynaecology; XI, Oto-, Rhino-, Laryngology; XII, Ophthalmology; XIII, Dermatology and Venereology; XIV, Radiology; XV, Tuberculosis. Section I will run to about 900 pages per year, Section II to about 1800, Section III to about 600. The subscription price for the two latter, which like this first Section, are of primary interest to biologists, are $45.00 and $15.00 per year, respectively. Each section is to be provided with both author and subject indexes.

EINFÜHRUNG IN DIE VERGLEICHENDE MORPHOLOGIE DER WIRBELTIERE.

By Adolf Portmann. Benno Schwabe & Company, Basel; Grune & Stratton, New York. S. Fr. 28.00. 335 pp.; ill. 1948.

This introduction to the comparative morphology of the vertebrates is scarcely a textbook, but is more of an essay, for it hits only the high spots and does not deal with minutiae. As such, it should prove of value to workers in other fields of biology, as well as in survey courses. It is well done by a person with thorough knowledge of this field, and it is extremely readable.

Portmann realizes that comparative anatomy, if it is to prosper, must take full cognizance of the findings of experimental embryology and genetics. Unfortunately, this realization is more evident in the introduction than throughout the body of the book. Admittedly, however, this is a difficult task in such a brief presentation as the present one.

In a short, introductory work there is a strong temptation to fill in the gaps in our knowledge and, for the sake of readability, not to differentiate too carefully between assumption and fact. That Portmann has largely succeeded in avoiding such pitfalls attests to the high character of the book. The reviewer spotted a few errors and doubtful statements, but these in no manner impair the general usefulness of the work. The illustrations, all in black-and-white, are uniformly excellent.

WILLIAM L. STRAUS, JR.

LES OUTILS *Chez les Êtres Vivants. L'Avenir de la Science—26.*

By Andrée Tétry. Gallimard, Paris. 550 fr. (paper). 345 pp.; text ill. 1948.

A. Tétry has obviously enjoyed drawing together these notes and the seventy-five clear diagrams and line sketches that illustrate her subject. She covers much territory, but the subject matter is arranged according to a rather novel viewpoint. Brief but separate mention is accorded teeth used for emergence from eggs and

cocoons, devices employed by animals in cleaning themselves, attachment structures in plants and invertebrates (but including also the angler fish's casting line), prehensile appendages, traps of all kinds, adopted homes such as shells on hermit crabs, mechanisms for gliding and parachuting (chiefly in seed dispersal), for diving and staying below the water surface or for floating on it. Organs for producing electricity, light, and sound come in for more extensive discussion. Other short chapters concern such devices as those for ejecting fecal pellets, and the use of foreign objects as tools. In the final chapter, the author has compared the devices used in one group of organisms with those in another, and has mentioned a number of examples where human mechanical ingenuity has duplicated designs found in living things. Each chapter ends with a useful bibliography, and there is an index to the scientific names of animals and plants discussed in the book. For those familiar with the genera concerned, the book makes light and interesting reading. It surveys the field of adaptations without becoming exhaustive on any point.

LORUS J. AND MARGERY J. MILNE

OCULUS: *Part I. Tabulae Biologicae, Volume XXII, Part I.*

Edited by K. Steindorff, F. P. Fischer, J. S. Friedenwald, and Arnold Sorsby. Dr. W. Junk, Amsterdam. F. 70.—; 60.—to subscribers (paper). viii + 408 pp. + 11 plates; text ill. 1947.

The familiar series of *Tabulae Biologicae* consists of compilations, without critical comment, of whatever biological data in the fields under consideration can readily be presented in tabular form. Judgment of the completeness of the various sections would require as many specialists as there are sections. The critical viewpoint of a single reviewer is necessarily limited to considering the apparent usefulness of the book to a biologist in search of detailed information.

The book under consideration, one of four parts of a volume to be devoted to the eye, is divided into seven sections: invertebrate eyes; metric and descriptive characteristics of animal and human eyes; embryology of the vertebrate eye; descriptive anatomy of the vertebrate eye, including man; the vertebrate pupil; intraocular pressure; and blood pressure of the eye vessels. The material is almost entirely tabular, with reference by number to the source articles listed at the end of each section. These number 1454 and are mostly from European journals. In five of the sections, tabular headings and incidental text are in German, in the remaining two in French. Each form considered is usually designated by the Linnaean binomial, but in some cases, such as domestic and other well known animals, the common name is used. There are nine pages of photographs and drawings pertaining to the orbit. Scattered throughout the text are line drawings and graphs.

The print is clear and carefully set. This book should be a useful source of quantitative data on the subjects covered and a useful bibliographic guide, particularly to those interested in the comparative aspects of our knowledge of the eye.

F. N. LOW

THE EPITHELIA OF WOMAN'S REPRODUCTIVE ORGANS. *A Correlative Study of Cyclic Changes.*

By George N. Papanicolaou, Herbert F. Traut and Andrew A. Marchetti. The Commonwealth Fund, New York; Geoffrey Cumberlege, Oxford University Press, London. $10.00. viii + 53 pp. + 23 plates + 1 chart. 1948.

In 1943, Papanicolaou and Traut published a small volume entitled *Diagnosis of Uterine Cancer by the Vaginal Smear.* This work marked an unusual step in the advance of medical science by presenting, largely through the medium of illustrations, the relationship of abnormal cell-types in the human vagina to malignancy in the female reproductive tract. The value of that publication has been attested by the confirmation of the work of those authors by numerous other investigators in the few years that have transpired since its publication and by the general adoption of the technic in the diagnosis of uterine cancer.

In the small volume here under discussion, these authors, to whom Marchetti must be added, have rendered a service which is surely equal to their first. Here is to be found the most complete pictorial record, in color, now available of the epithelial structures of the human female generative tract. In all, there are twenty-three plates, all but two of which are in color. The magnificently executed color plates consist of colored photomicrographs complemented by colored drawings by the artist, Hashime Murayama. When it is necessary to show details, drawings are inserted with or between the colored photographs. Four of the plates, consisting of nine figures, deal with the endometrium under different conditions and with cell-types observed in endocervical smears. Two plates, with a total of ten figures, deal with the endocervical glands. Eight plates, with forty-four figures, deal with the epithelial structures of the vagina, and with the problem of cell-types in vaginal smears. A large correlative chart, likewise in color, serves to bring together the principal facts pertaining to the cyclic changes in the tissues just described and to present them in relation to hormone excretion curves and the typical basal body temperature curve of the menstrual cycle.

The brief text which precedes these illustrations provides some—but not much—amplification of the legends to the figures themselves. The attempt to present in so short a space a description in words of the

changes that occur in these tissues in various reproductive states must of necessity be less than satisfactory. It leaves unsaid much that ought to be said, and it has in the case of this text permitted a few small, but definite errors to be admitted in the absence of sufficient qualification.

These faults are small in proportion to the success which has been achieved. All who are concerned with gynecic problems will find this book helpful if not essential. It will be of unquestioned value to those who must teach technical aspects of the human female reproductive cycle, and by the same token, it should be of great value to those students of medicine who are preparing for careers in obstetrics and gynecology.

S. R. M. REYNOLDS

TEXTBOOK OF ANATOMY AND PHYSIOLOGY. *Twelfth Edition.*

By Diana Clifford Kimber and Carolyn E. Gray; revised by Caroline E. Stackpole and Lutia C. Leavell. The Macmillan Company, New York. $4.00. x + 773 pp.; ill. 1948.

This book, popular with educated laymen and beginning students since 1902, needs little introduction. There has been some rearrangement of material in this 12th edition. Chapter 3 particularly emphasizes topics on the physiology and biochemistry of the cell; chapter 24, an especially excellent one, now includes the section on fetal circulation. Many of the illustrations have been completely or partly revised, and the number has been increased to 360. Ample historical references are made throughout the text, and the bibliography of reference books is suitably selected for the beginning student.

DAVID B. TYLER

OCCUPATIONAL MARKS AND OTHER PHYSICAL SIGNS. *A Guide to Personal Identification.*

By Francesco Ronchese; foreword by John G. Downing. Grune & Stratton, New York. $5.50. xvi + 181 pp.; ill. 1948.

On first picking up this fascinating little book, readers of Conan Doyle will be reminded of a reference made in his "Sign of Four" to a mythical monograph by Sherlock Holmes, "Upon the Influence of a Trade upon the Form of the Hand (with lithotypes of the hands of slaters, sailors, cork-cutters, compositors, weavers, and diamond-polishers)." In the field of forensic medicine and criminology relatively little attention has been paid to the value of acquired markings as an aid in making personal identification. Furthermore, it appears that there has been no adequate review of the literature on this subject. In this interesting book, Ronchese has shown how occupational marks, due to habits and hazards of work, accidents, operations, and diseases can be very useful to physicians or those whose livelihood depends upon the detection of crime. In addition, he has made what appears to this reviewer to be the first comprehensive survey of the literature in this field. An excellent job has also been done in correlating and classifying the various stigmata due to occupational trauma or disease, and for this Ronchese has drawn on his own experience, as well as upon the literature available on this subject. The book is profusely illustrated, over 100 pages of this 181-page text being devoted to photographs and drawings. The book has what appears to be a rather complete bibliography. It should serve as a source book and guide for all students of the subject and will no doubt stimulate further investigation in this field. Physicians, students, and workers in this and related fields will find the book a valuable addition to their library.

DAVID B. TYLER

ANIMAL PHYSIOLOGY

ANNUAL REVIEW OF PHYSIOLOGY. *Volume X.*

Edited by Victor E. Hall, Jefferson M. Crismon, and Arthur C. Giese. Annual Reviews and American Physiological Society, Stanford. $6.00. xii + 552 pp. 1948.

In spite of the Editors' express desire, only four reviews in the current volume of this series are contributed from overseas, and all of these come from the British Commonwealth of Nations. In fact, one of these (by Lee) can scarcely be counted as foreign, since the author is now on permanent appointment in this country. It is to be expected, however, that the Editors will shortly succeed in their efforts to make the reviews truly international in authorship. All scientists must be concerned, as they awake to the existing situation, about the division of the nations of the world into separate camps, between which the flow of scientific information is diminished if not quite shut off, while within each camp the free discussion of scientific matters is restricted by considerations of military security. An examination of the literature cited in the present contributions is enough to impress one with the reality of this threat to science.

The contributors represent an outstanding selection, and it may be wishful thinking on the reviewer's part, but it seems to him that the reviews are more critical than formerly and less mere compendia. The contents are as follows:

Physical Properties of Protoplasm (F. O. Schmitt and A. R. T. Denues); Physiological Aspects of Genetics (G. W. Beadle); Developmental Physiology (J. S. Nicholas); Physiology of Reproduction (S. R. M. Reynolds); Conduction and Synaptic Transmission in the Nervous System (J. C. Eccles); Somatic Functions of the Central Nervous System (Warren S. McCulloch and Jerome Y. Lettvin); Vision (A.

Chapanis); Digestive System (C. C. N. Vass); Blood Cytology (Edwin E. Osgood); Heart (John McMichael); Peripheral Circulation (Benjamin W. Zweifach); Metabolic Aspects of Shock (Alfred E. Wilhelmi); Respiration (Fred A. Hitchcock and Wm. V. Whitehorn); Anoxia in Aviation (Leslie F. Nims); Regulation of Energy Exchange (John R. Brobeck); Metabolic Functions of the Endocrine Glands (Evelyn Anderson); Heat and Cold (Douglas H. K. Lee); Physiology of Sweating (Carl F. List); Pharmacology (E. M. K. Geiling); The Coagulation of Blood (H. P. Smith and Jos. E. Flynn); Hemostasis (Virginia K. Frantz); Physiological Psychology (W. Horsley Gantt); Physiological Effects of Radiant Energy (R. Lowry Dobson and John H. Lawrence).

The Board has adopted a new procedure in the selection of future contributors. Present contributors submit the names of those they consider most competent to prepare reviews on the same subject; Board members, Editors, and others may add to this list; and selections are then made from the final list, as a rule. The Board invites interested biologists to make nominations for contributors at any time.

BENTLEY GLASS

THE HUMAN BODY AND ITS FUNCTIONS: *an Elementary Textbook of Physiology. Revised Edition.*

By C. H. Best and N. B. Taylor. Henry Holt and Company, New York. $3.60. xii + 500 pp. + 8 plates; text ill. 1948.

This revised edition of an Elementary Textbook of Physiology, written in a simple style, is designed essentially for the layman or student with a minimum of scientific training. It has a pronouncing glossary and is well illustrated with numerous figures, photographs, and diagrams.

DAVID B. TYLER

TEXTBOOK ON HUMAN PHYSIOLOGY.

By William F. Hamilton. F. A. Davis Company, Philadelphia. $6.00. xvi + 504 pp.; ill. 1947.

Since the reviewer feels that no limit should be placed upon a student's ambition to become a scientist and to realize the complexity of the subject matter of physiology, he is not in full accord with the sentiments expressed by the author in the preface. In the text of this book very little space is devoted to the description of evidence supporting the conclusions drawn or the methods used in obtaining this evidence. Thus a student who uses this book exclusively would gain little in ability to judge the validity of statements and to differentiate between conclusions well supported and those weakly supported by evidence. No doubt this volume might appeal to some students, and it would serve well as an introductory treatment of the subject, but it is the author's experience that the better students frequently wish to know more than is incorporated in even the more detailed textbooks. A superior view of teaching requirements is one which visualizes a greater ability on the part of the student. It is likewise felt that many common questions could not be answered in a very satisfactory fashion by one who relied solely on this volume. Although it describes phenomena very clearly and covers most of the field of physiology, it is not to be recommended as a textbook for medical students.

CHANDLER McC. BROOKS

MICROCALORIMETRY.

By W. Swietoslawski. Reinhold Publishing Corporation, New York. $4.75. x + 199 pp.; ill. 1946.

This small volume describes the work performed by the author and his associates during the several years they were engaged in developing their techniques of microcalorimetry. For the most part it is a highly technical account of methodology. Only those well acquainted with this particular field of microcalorimetry will profit much from perusal of this book, but it impresses even the uninformed as being a very useful product, containing much which will be of interest to those sufficiently well equipped to understand its basic concepts.

CHANDLER McC. BROOKS

BLOOD DERIVATIVES AND SUBSTITUTES. *Preparation, Storage, Administration and Clinical Results Including a Discussion of Shock, Etiology, Physiology, Pathology and Management.*

By Charles Stanley White and Jacob Joseph Weinstein. The Williams & Wilkins Company, Baltimore. $7.50. xviii + 484 pp.; ill. 1947.

The entire presentation of the subject is on an elementary level. The discussions of the physiological and biological bases for the use of the blood substitutes are superficial summaries of standard reports. The chapters dealing with the technical phases of the preparation and administration of blood derivatives and blood substitutes are in great detail and replete with illustrations taken from the brochures of the manufacturers of equipment for this purpose.

MARK M. RAVITCH

NATURE OF LIFE. *A Study on Muscle.*

By A. Szent-Györgyi. Academic Press, New York. $3.00. 96 pp. + 7 plates; text ill. 1948.

This small volume contains four lectures delivered at the University of Birmingham and the Massachusetts Institute of Technology. In the first chapter, the

author deals with the histological, molecular, and electronic structure of muscle. He discusses the nature of the fiber striations, and the composition and mechanism of contraction of actomyosin. The second chapter is devoted to a more detailed consideration of myosin and actin. The third section of the book deals with actomyosin, which results from the reaction of myosin and F-actin. The geometry, energy production, and energy consumption of contraction are discussed. Finally, in chapter four, the activity of muscle fibers is described and explanations given. This is a most inspiring little treatise, composed partly of experimental material and partly of philosophical conclusions. It expresses the spirit of its colorful and admirable author.

CHANDLER McC. BROOKS

VISION AND THE EYE. *Frontiers of Science Series.*
By M. H. Pirenne. The Pilot Press, London. 12s. 6d. xx + 187 pp. + 7 plates; text ill. 1948.

This is a well-written elementary textbook on the physiology of vision. The words "physiology of vision" are used advisedly here, because the author has meticulously minimized or avoided any discussion of the subjective phenomena of vision—the sensations and perceptions of the psychologist. Neurological, electrophysiological, and quantum studies are stressed throughout. There are also some good sections on the comparative anatomy of the eye and a chapter on The Eyes and the Vision of Insects.

The visual expert will find nothing that is new to him in this book, but the student should find it a concise summary of essential facts. Although one could hardly call a book as brief as this well-rounded, it does maintain a careful balance of important topics in its contents. The author's style of writing is particularly lucid. He has been able, for example, to describe clearly the substance of recent complicated experiments on the number of light quanta necessary for vision. The illustrations (99 figures plus 4 colored plates) also deserve special mention. In general, they are clear and very helpful in illustrating difficult points in the text.

A. CHAPANIS

SENSORY MECHANISMS OF THE RETINA, *With an Appendix on Electroretinography.*
By Ragnar Granit. Geoffrey Cumberlege, Oxford University Press, London, New York, and Toronto. $11.00. xxiv + 412 pp.; ill. 1947.

All too frequently a treatise on some aspect of physiology bears a title which implies that the subject is presented without phylogenetic restrictions, yet examination reveals that the study has been limited to man, or to mammals, or vertebrates only. *Sensory Mechanisms of the Retina* is a presentation which escapes this fault because it is concerned with retinal phenomena in all animals of which sufficient investigation has been undertaken to contribute to an overall understanding of the basic mechanisms of complicated photoreceptors. It should, therefore, interest not only students of vision but biologists and physiologists in general. It is divided into four sections: (1) the electrical signs of excitation and inhibition in the retina and optic nerve; (2) the properties of rods and cones; variations in area, intensity, and duration of the stimulus in dark and light adaptation; flicker; (3) the properties of the photosensitive substances and the mechanism of excitation; (4) color reception. Granit introduces his subject by discussing the nerve as a model sense organ. This is followed by a consideration of the electroretinograms and optic nerve responses of simple invertebrate eyes. This section, incidentally, constitutes a rather stimulating, if short, review of the important work on invertebrates. It is noteworthy that throughout the book the author integrates data from all sources well. Thus, such topics as the resting potential, duration, and intensity of stimulus, adaptation, and sensitivity curves are discussed from the point of view of all organisms which have been investigated. The assimilation of material has been accomplished in a highly critical manner. On occasion, the discussions generate a detectable amount of heat, as in an exchange of words with Wulff. Although the book is intended primarily as a presentation of factual material, it does not escape the necessity of including a fair share of theoretical discussion. This is a scholarly, comprehensive, and thought-provoking account of a complicated branch of neurophysiology. It is well documented and excellently illustrated.

V. G. DETHIER

ANIMAL NUTRITION

FOOD PRODUCTS. *Fourth Edition.*
By Henry C. Sherman. The Macmillan Company, New York. $4.00. viii + 428 pp.; ill. 1948.

This edition of a book written for students of agriculture is arranged essentially as in the third edition. A typical chapter has four main parts: "(1) an account of the production of food in question and its preparation . . . (2) questions of sanitation, inspection and standards of purity . . . (3) the general composition, mineral content and vitamin values and (4) special characteristics, digestibility, nutritive value. . . ." The author assumes that the student will have for ready reference the U. S. Dept. of Agriculture's Miscellaneous Publication 372, and so has shortened the tables dealing with food

composition. The book is well indexed. A generous bibliography that contains many papers published between 1942 and 1947 is found at the end of each chapter. The appendix includes a simple method for the statistical treatment of data. In addition to its value to students of agriculture, the book will be of interest to the biologist as a reference on the composition and preparation of some common food products.

DAVID B. TYLER

FOLIC ACID. *Annals of The New York Academy of Sciences, Volume XLVIII, Article 5.*

By Y. SubbaRow, Robert B. Angier, Nestor Bohonos, James H. Boothe, Mary C. Clark, Donna B. Cosulich, Raymond W. Cunningham, Floyd S. Daft, M. J. Fahrenbach, Ben King Harned, Robert W. Heinle, M. E. Hultquist, Brian L. Hutchings, Erwin Kuh, John H. Mowat, Evelyn M. Nelson, H. V. Nelson, E. H. Northey, W. H. Peterson, Doris R. Seeger, Joseph Semb, J. P. Sickels, Nathan Sloane, Helen D. Smith, James M. Smith, Jr., Tom D. Spies, E. L. R. Stokstad, John R. Totter, Coy W. Waller, and Arnold D. Welch. The New York Academy of Sciences, New York. $1.50 (paper). Pp. 255–350 + 10 plates; text ill. 1946.

FOLIC ACID. SYNTHESIS OF PTEROYLGLUTAMIC ACID (LIVER *L. casei* FACTOR) AND PTEROIC ACID—PART II. *Annals of The New York Academy of Sciences, Volume XLVIII, Article 5 (Supplement).*

By M. E. Hultquist, Erwin Kuh, Donna B. Cosulich, M. J. Fahrenbach, E. H. Northey, Doris R. Seeger, J. P. Sickels, and James M. Smith, Jr., and Robert B. Angier, James H. Boothe, Brian L. Hutchings, John H. Mowat, Joseph Semb, E. L. R. Stokstad, Y. SubbaRow, and Coy W. Waller. The New York Academy of Sciences, New York. (Paper). vi pp. 1947.

The first bulletin mentioned contains papers by thirty-one active investigators. It covers the history of the folic acid factors, isolation from liver of the *L. casei* factor, its degradation, structure, and synthesis of the pteridine degradation products; synthesis of pteroylglutamic acid and pteroic acid; pharmacological studies of pteroylglutamic acid; physiological aspects; vitamin M deficiency; observations on the therapeutic usefulness of synthetic folic acid; the role of conjugated and free forms of folic acid in the control of pernicious anemia; clinical observations; and biochemical aspects. The articles were written by outstanding investigators in their fields. The supplement on synthesis is adequately described by its title. Both of these publications are of permanent value as records of the state of knowledge at the date of their publication.

E. V. MCCOLLUM

BIOPHYSICS

ADVANCES IN BIOLOGICAL AND MEDICAL PHYSICS. *Volume I.*

Edited by John H. Lawrence and Joseph G. Hamilton. Academic Press, New York. $8.60. xii + 484 pp.; ill. 1948.

This volume consists of ten reviews. Dougherty and Lawrence discuss heavy and radioactive isotopes in clinical and experimental medicine; Vennesland, nitrogen and carbon isotopes: their application *in vivo* to the study of the animal organism; Cohn, the nature and production of artificial radioactivity; Evans, fundamentals of radioactivity and its instrumentation; Parker, health-physics and radiation protection; Hahn, the use of radioisotopes in iron and hemoglobin metabolism; Chaikoff and Zilbersmit, application of radiophosphorus to study of phospholipid metabolism; Leblond, iodine metabolism; Howland and Warren, effects of atomic bomb irradiation on the Japanese; and G. Hevesy, nucleic acid metabolism.

L. B. FLEXNER

BIOCHEMISTRY

TOPICS IN PHYSICAL CHEMISTRY. *A Supplementary Text for Students of Medicine.*

By W. Mansfield Clark. The Williams & Wilkins Company, Baltimore. $10.00. xvi + 738 pp.; ill. 1948.

Research students of medicine, biology, and biochemistry now require, in certain phases of their work, even more physical chemistry than may be obtained in the usual undergraduate course. Numerous physical chemistry books for students of biology and medicine have been written to meet some of these needs. Unfortunately, it seems, most of these books are designed for short introductory courses to the subject. It is easy to understand, therefore, why none of them has completely met the need. Clark, in his excellent book, forgets about courses and the experts and writes a book in an effort "to meet the diverse needs of the medical student." Fortunately, in so doing, he has given not only the students of medicine but also the students of biology and biochemistry a physical chemistry book which is understandable and useful at the research level.

The unity in presentation of the material covering both the theoretical and practical aspects of various subjects is excellent. For example, at the end of Chapter 4, which deals with volumetric analysis, a brief note of caution is inserted for those workers who use pipettes for transferring infectious agents. Again, in Chapter 5, after considering density, the theory, use, and application of the Cartesian diver are discussed. The problems of sedimentation and the centrifuge are considered in Chapter 6. In Chapters 7 through 11 an integrated discussion of the properties of molecules is

presented in the following sequence: first, consideration is given to the gas laws, in which manometric methods are discussed, then properties of solution, distribution between phases, diffusion, and finally the problem of permeability. The law of mass action, including rates and equilibria of various reactions, is considered in Chapters 12 and 13, followed by a discussion of electrolytes (Chapter 14) and of conductance by electrolytes (Chapter 15). The various principles elaborated in the foregoing chapters are used as a basis of discussion of proton exchanges (Chapter 16), protein solutions (Chapter 17), and equilibrium states of blood electrolytes (Chapter 18).

The general subject of thermodynamics, usually omitted in abbreviated texts of physical chemistry for students of biology and medicine, is presented in a clear and lucid manner in Chapters 19 and 20. Although the student may have some difficulty in grasping the concept of entropy change in reactions at first, if he follows the author's development of the subject he will finally acquire the understanding necessary for the practical use of the relationships between free energy, heat, and entropy changes. It would have been of value to consider at this point the energy relationships in biological systems, especially the phosphate esters and free energy change.

The important topic of oxidation-reduction systems is developed in Chapter 21, and is followed by a consideration of the glass electrode, potentiometry (Chapter 22), and the polarograph (Chapter 23). In the seven remaining chapters one may find excellent introductions to such subjects as atomic structure, isotopes, refraction and double refraction, stereoisomerism, spectrophotometry, emission and absorption of radiant energy, luminescence, and finally a few topics from colloid chemistry.

Among the numerous useful appendixes may be mentioned those devoted to elementary mathematics, symbols and formulas, constants and conversion factors, definitions, and a table of logarithms. *Topics in Physical Chemistry* is a valuable book and is recommended to students of both biology and medicine.

WILLIAM D. MCELROY

INTRODUCTION TO PHYSICAL BIOCHEMISTRY. *Second Edition.*

By J. M. Johlin. Paul B. Hoeber, Medical Book Department of Harper & Brothers, New York. $3.75. xii + 246 pp.; ill. 1949.

The first edition of this book presented an elementary survey of a few aspects of physical chemistry applied to a number of topics in biochemistry, particularly the physical biochemistry of blood. The major change in the new edition is an expansion of a chapter on oxidation-reduction systems.

While the volume is eminently readable and attractively printed, the presentation is diffuse and, in many instances, imprecise. A descriptive approach is adopted throughout. It is difficult to see how material presented in this way can entirely avoid generating some confusion in the minds of novitiates. In Chapter I, for instance, one finds a discussion of water which omits completely any mention of the hydrogen bond, although the formulae pictured for water polymers make no sense without such a bond. Incidentally, the statement that three stable hydrogen isotopes exist (p. 9) appears to have survived two editions. Throughout the text, statements of an over-simplified nature abound. An instance is a remark that photosynthetic CO_2 assimilation seems to represent a reversal of the pathway followed in carbohydrate metabolism (p. 211).

It seems to the writer that a student desirous of obtaining a solid grounding in physical biochemistry should supplement his study of this book with that of others, in particular the excellent volume by H. B. Bull which appeared in 1943.

MARTIN D. KAMEN

INTRODUCTION TO ORGANIC AND BIOLOGICAL CHEMISTRY. *Second Edition.*

By L. Earle Arnow and Henry C. Reitz. The C. V. Mosby Company, St. Louis. $5.75. 795 pp.; ill. 1949.

In the second edition of this elementary textbook combining organic chemistry and biochemistry, the authors have retained their emphasis on material of interest to students in premedical and associated courses. Among sections which have been rewritten and enlarged are those that deal with petroleum, the chemistry of starch, and the structure of pectin. Much new material reflecting wartime research on antimalarials, fluorine compounds, silicones, mercaptans, penicillin, etc., has been added. There are 30 chapters, each with an excellent set of study questions.

Although the authors have intended this textbook primarily for use by students in the biological sciences, the presentation of elementary organic chemistry appears sufficiently thorough to warrant its use as a general textbook in elementary organic chemistry. Few textbooks at this level contain such handy material on substances of biological interest.

MARTIN D. KAMEN

HUMAN BIOCHEMISTRY. *Second Edition.*

By Israel S. Kleiner. The C. V. Mosby Company, St. Louis. $7.00. 649 pp. + 5 plates; text ill. 1948.

This textbook of biochemistry, designed especially for

medical and dental students, has been revised to include a new short chapter, Chemical Structure in Relation to Biological Phenomena. This new section deals briefly with some mechanisms in detoxication and biochemical antagonism. The appendix contains data on the preparation of standard buffer solutions and rather complete tables on the composition of various foods. The book is well indexed, and the bibliography is selected primarily to meet the needs of the beginning student.

DAVID B. TYLER

PHYSIOLOGICAL CHEMISTRY. *Seventh Edition.*

By J. F. McClendon. The C. V. Mosby Company, St. Louis. $4.25. 463 pp.; text ill. 1946.

This textbook was designed for college teaching of physiological chemistry. Though presupposing that students using it already possess a knowledge of general and organic chemistry, it is not highly technical in its treatment of the subjects presented. Certain fundamentals of physical chemistry, such as the gas laws, laws of solutions, and the effects of temperature and hydrogen ion concentration on speed of reactions are dealt with. The metabolism of inorganic compounds, fats, phospholipins, carbohydrates, and proteins is treated at length. Enzymes, vitamins, internal secretions, urine, and blood are dealt with in Part II of the volume. There is also a special chapter describing experiments of particular interest to dental students. A list of references, a glossary and tables of International Atomic Weights are appended. Although the material is clearly presented, the tone is not that of a modern book, nor are many of the references of recent date.

CHANDLER McC. BROOKS

ISOTOPIC CARBON. *Techniques in Its Measurement and Chemical Manipulation.*

By Melvin Calvin, Charles Heidelberger, James C. Reid, Bert M. Tolbert, and Peter F. Yankwich. John Wiley & Sons, New York; Chapman & Hall, London. $5.50. xiv + 376 pp.; ill. 1949.

This valuable reference volume discusses authoritatively instruments for the measurement of radioactivity, the preparation of samples of isotopic carbon for measurement, vacuum techniques in organic chemistry, synthesis of carbon-labelled compounds, criteria of purity, degradation procedures, and biosynthetic methods. Details are given of every synthesis with isotopic carbon reported up to the time of writing, and the bibliography contains all published tracer applications of isotopic carbon.

L. B. FLEXNER

TECHNIQUES OF HISTO- AND CYTOCHEMISTRY. *A Manual of Morphological and Quantitative Micromethods for Inorganic, Organic and Enzyme Constituents in Biological Materials.*

By David Glick; foreword by Robert R. Bensley. Interscience Publishers, New York and London. $8.00. xxiv + 531 pp.; ill. 1949.

A manual of morphological and quantitative micromethods for the examination of inorganic, organic, and enzymic constituents in biological material, this volume meets a long-felt need. The author has assembled in full detail the original descriptions of the principal procedures in their most effectively modified forms. The magnitude of this undertaking is indicated by the bibliography of something over 600 titles dealing with apparatus and techniques.

The opening section deals primarily with the major modifications of "morphologic" or physical-microscopic and chemical-staining methods which have appeared since Lison's *Histochemie Animale* of 1936. The author's cautions regarding the requirements such methods should meet constitute brief but excellent advice in a field where it cannot be overemphasized that "on the user rests the responsibility for avoiding the many pitfalls which are inevitable."

Most of the book deals with quantitative analytical procedures which have been applied, for the most part successfully, in the assay of cells and tissues. In view of the limited circulation of much of the original literature, the extensive presentation of the Carlsberg methods is especially valuable. The presentation is intentionally non-critical, but is selective rather than encyclopedic, and the bibliography is almost complete to 1947. The liberal use of diagrams greatly clarifies the text. For anyone concerned with procedures of chemical analysis in the cubic millimeter range, this is a valuable reference manual of techniques rather than a textbook. Evaluations of principles are accordingly minimized, and the coverage possesses some of the merits as well as the defects of single authorship. The closing sections on deductive methods and on centrifugal separation of cellular components are summarily treated. Chromatographic methods are omitted.

For the limited but increasing number of biologists who are engaged in the identification, localization, and quantitative assay of microscopic samples of tissue, this volume may well become a vade mecum. It should incidentally aid in decreasing further republication in the journals of numerous previously described gadgets used in this field, especially respirometers and burettes.

As a survey of the methodology, the book is weakest just where the methods are most primitive, namely, in the problems of sampling the biological material. To the reviewer it is evident that the quantitative uncertainties in the biological sample in general far exceed

the limits of error of the available quantitative methods. Nevertheless, further orders of magnitude of refinement of the chemical methods will be required before the analyses of single cells can regularly be achieved in any but special circumstances.

WM. L. DOYLE

CHEMISTRY OF THE CARBOHYDRATES. *By William Ward Pigman and Rudolph Maximilian Goepp, Jr. Academic Press, New York City.* $10.80. xviii + 748 pp.; ill. 1948.

Since E. F. Armstrong and K. F. Armstrong's last edition of the monograph of *The Carbohydrates* appeared in 1934, there has been no adequate treatment of this subject in the English language. In the fourteen-year period that has elapsed since then a vast amount of new knowledge has accumulated in the field of carbohydrate chemistry. The appearance of Pigman and Goepp's book is therefore timely and fulfils a definite need. It is the most comprehensive one-volume treatise now available dealing with all phases of carbohydrate chemistry, including monosaccharides, oligosaccharides, polysaccharides and derived products. The subject is handled in a scholarly manner, and the presentation is logical and concise. The chemistry of the carbohydrates is considered from the organic, physical-chemical, biological, and industrial points of view. The main emphasis, however, is laid on the structure, synthesis, and stereochemistry of these compounds.

The fifteen chapters which constitute this book bear the following titles: (I) Introduction; (II) Structure and Stereochemistry of the Monosaccharides; (III) Occurrence, Properties, Synthesis and Analysis of the Monosaccharides; (IV) Esters; (V) Glycosides, Full Acetals and Thioacetals; (VI) The Polyols; (VII) Acids and Oxidation Products of Carbohydrates; (VIII) Ethers, Anhydrides and Unsaturated Derivatives; (IX) Nitrogenous Derivatives; (X) Oligosaccharides; (XI) Naturally Occurring Glycosides and Glycosidases; (XII) Classification and Determination of Structure of the Polysaccharides; (XIII) Cellulose; (XIV) The Starches and Starch Substances; (XV) Polyuronides, Hemicelluloses, Plant Gums, Microbial Polysaccharides and Related Substances.

There is a wealth of references to the original literature, an author's index and an extensive subject index. Ample illustrations are given throughout the book through the media of graphs, tables, and structural formulas. Pigman and Goepp's treatise is an invaluable contribution to chemists and biochemists interested in carbohydrate chemistry, whether they are teachers, students, or research workers.

W. Z. HASSID

ADVANCES IN CARBOHYDRATE CHEMISTRY. *Volume 3. Edited by W. W. Pigman and M. L. Wolfrom. Academic Press, New York.* $8.50. xxiv + 424 pp. 1948.

As in the previous volumes of this series, certain aspects of carbohydrate chemistry have been critically examined and interpreted by carefully selected experts.

In accordance with the custom established by the two preceding volumes, the first article is written by C. S. Hudson. Its title is Historical Aspects of Emil Fischer's Fundamental Conventions for Writing Stereo-Formulas in a Plane. This article is a most lucid presentation of the reasons which led Emil Fischer to introduce the system of writing configurational formulas, based on the principles of van't Hoff-LeBel's theory of the asymmetric carbon atom now in universal use. Until the appearance of this article there was no adequate presentation of this subject. As Hudson says, "... the subject is treated so cursorily in most college text-books, even when mentioned at all, that few students today understand the historical background and the origin of our present conventions for writing the stereo formulas of general organic chemistry."

In the second article, The Structure and Reactivity of the Hydralzone and Osazone Derivatives of the Sugars, E. G. V. Percival considers the various structural forms of the sugar hydrazones and osazones. Although the hydrazine derivatives of sugars are assumed to be open chain structures, there is evidence that they may also exist in the cyclic form. Theories of osazone formation are also discussed.

The Chemistry and Configuration of Cyclitols, by Hewitt G. Fletcher, deals with the chemistry and configuration of the hexahydroxycyclohexanes (inositols) and some of the closely related polyhydroxycarbocyclic substances. A comprehensive article by B. Helferich is devoted to Trityl Esters of Carbohydrates. It contains a complete table listing all the derivatives and their structures, specific rotations and melting points.

An interesting discussion on the Glutose and the Unfermentable Reducing Substances in Cane Molasses is presented by Louis Stattler. This author points out that "sugar technologists have generally accepted the belief that D-glucose and D-fructose in cane juice are partially converted into D-mannose, D-psicose and glutose during the alkaline clarification process, but notwithstanding all efforts during the past fifty years, glutose itself has remained a mysteriously elusive substance which has defied isolation and characterization." It is concluded that the "term 'glutose' as applied to the unfermentable reducing sugars or cane molasses is ambiguous and misleading." The products which have been isolated from such unfermentable reducing substances include anhydro sugars and minor amounts of simple sugars.

In an excellent article entitled The Halogen Oxidation of Simple Carbohydrates Excluding the Action of

Periodic Acid, John W. Green has critically reviewed the great amount of work done on this subject. He cites some 180 references. In dealing with the Molecular Constitution of Cellulose, Jack Compton calls attention to the fact that in recent years considerable evidence has accumulated indicating the inadequacy of certain accepted ideas regarding the structure of cellulose. He reviews the "foundations on which conventional ideas are based" and attempts to incorporate some of the new points of view so that they may become a part of a broader concept. Samuel Gurin, in an article on Isotopic Tracers in the Study of Carbohydrate Metabolism, discusses the rather intricate subject of intermediary carbohydrate metabolism in living cells, pointing out that the metabolic pathways of carbohydrates are interwoven with those of fats and proteins in a most complex manner. In discussing the Products of Enzymatic Degradation of Starch and Glycogen, Karl Myrbäck considers the structure of these polysaccharides based on studies of enzymatic breakdown by amylases. The article is based chiefly on the author's own work. Inasmuch as most of his original work has been published in Swedish, the article is useful in providing English-speaking workers an opportunity to get acquainted with the Swedish school of thought on that subject.

Stacey and Kent discuss the structure as well as the biological properties of the Polysaccharides of *Mycobacterium tuberculosis*. The concluding article, on The Chemistry of Streptomycin, by R. U. Lemieux and W. L. Wolfrom, deals with the isolation, purification, and determination of structure of the antibiotic streptomycin, which is considered as second only in importance to penicillin.

Approximately one-third of the volume is devoted to biochemical subjects. For this reason the book will be of interest equally to chemists and biochemists.

W. Z. HASSID

CELLULOSE CHEMISTRY.

By Mark Plunguian. Chemical Publishing Company, Brooklyn, New York. $2.25. viii + 97 pp. 1943.

As stated by the author in his preface: "This book was designed primarily as a brief and up-to-date introduction to the chemistry of cellulose for non-cellulose chemists." Since the publication of this brief treatise several extensive and excellent monographs on the subject have been prepared, namely, *Cellulose and Cellulose Derivatives* by Emil Ott (1943), *Cellulose Chemistry* by Emil Heuser (1944), and *Wood Chemistry* by Louis E. Wise (1944).

Plunguian's book contains eight chapters, starting with a historical introduction pertaining to the development of cellulose chemistry, its early recognition as a main constituent of the cell wall and its relation to the "hemicelluloses" and lignin. Chapter 2 describes the microscopic and submicroscopic structure of wood and cotton fibers, with the aid of a number of excellent photographs. The third chapter is concerned with methods of the isolation and purification of cellulose by various processes, and chapter 4 gives a brief summary of the general physical properties of cellulose. In chapter 5 are discussed methods of cellulose dispersion in various solvents, technical procedures employed for spinning films, and the process of mercerization. Then follow two chapters dealing with important cellulose derivatives, cellulose ethers, esters of nitric and organic acids, and the modified celluloses, such as cellulose hydrate, hydrocellulose, and oxycellulose. The last two chapters are devoted to the more theoretical aspects of chemical structure of cellulose, i.e., its molecular constitution, molecular weight, and micellar structure as interpreted by x-ray data. It is regrettable that the author does not mention the work of Sponsler and Dore, whose classical x-ray study was a turning point in our modern conception of the structure of cellulose.

Thd book is beautifully printed and edited, and is illustrated with excellent photographs, diagrams, and perspective formulas. The author has succeeded in producing a popular, well written, and concise summary of the subject.

W. Z. HASSID

FATTY ACIDS: *Their Chemistry and Physical Properties. Fats and Oils: A Series of Monographs on the Chemistry and Technology of Fats, Oils, and Related Substances.*

By Klare S. Markley. Interscience Publishers, New York and London. $10.00. x + 668 pp.; ill. 1947.

Quite aside from its obvious value to chemists, this volume should be of considerable interest to biologists. As a reference work it is complete and authoritative. The subject of fatty acids is treated under six headings: the nature and history of fats and waxes, classification and structure of the fatty acids, their physical properties, their chemical reactions, their synthesis, and their isolation and identification. Of particular value to biologists is the section which treats of the physical properties of fatty acids. Included here are detailed discussions of crystal, spectral, thermal, and solubility properties.

This book is well documented and supplied with a generous number of tables, graphs, and structural formulae.

V. G. DETHIER

ADVANCES IN PROTEIN CHEMISTRY. *Volume IV.*

Edited by M. L. Anson and John T. Edsall. Aca-

demic Press, New York. $8.50. x + 575 pp.; ill. 1948.

The high standard of previous volumes in this series has been maintained in the present volume. The editors have assembled eight excellently written, illuminating, and critical articles.

The first article, on protein gels, by Ferry, contains in addition to a good review of factual material on protein gelation in networks, a sound discussion of present notions and problems involved in general theories of gel structure. A special section on gelatin is included. The succeeding articles by Putnam, Pappenheimer, and Gutman appear most adequate and for the most part clearly written, even though the material covered is less amenable to the kind of presentation used by Ferry. The article on electrophoretic preparation of biological material by Svensson is an admirable mate to the article by Tiselius on adsorption analysis which appeared in Volume III of this series.

In the sixth article, Neuberger presents an extensive and exhaustive treatment of evidence relating to configurations of amino acids, reactions involving substitutions in the α-carbon atom, catalytic racemization, and perhaps most interesting, the occurrence and distribution of d-amino acids in nature. Following this, there is a concise and admirably coherent discussion by Corey of the structure of certain amino acids and peptides, as revealed by x-ray studies. The diagrams in association with these data are more than usually clear. The volume ends with a scholarly presentation by Wyman of present knowledge relating to porphyrin-protein complexes, special emphasis being given to the relation between structure (distribution and character of heme and protein) and function.

The paper in this volume is of higher quality than in previous volumes, so that there has been an improvement in legibility and "feel."

MARTIN D. KAMEN

MICROBIOLOGY

THE BIOLOGY OF BACTERIA. *An Introduction to General Microbiology. Third Edition.*

By Arthur T. Henrici; revised by Erling J. Ordal. D. C. Heath and Company, Boston, Chicago, Dallas, Atlanta, New York, San Francisco, and London. $5.50. xvi + 577 pp.; ill. 1948.

Compared with the second edition, published some 10 years ago, before the death of the original author, this third edition of an excellent textbook includes many additions and revisions of material. It may be used as a sole textbook by those who take only one course in microbiology, or as an introductory textbook for those who will later pursue the subject further. As before, it contains a large amount of good solid bacteriological material, soundly handled and interpreted. The book will be of maximum value to students with an excellent grounding in chemistry and general biology. Those with less preparation may find it a trifle too advanced.

Some minor criticisms are in order. Very few references are given, and then only as footnotes. Although various investigators are mentioned by name in connection with their contributions, the dates of their work are given in only a minority of cases. Thus the student has but little idea of *when* the work was done. Although well up to date in many respects, the book contains surprisingly little information on antibiotics, such as penicillin and streptomycin. The index contains one reference to antibiotics (p. 216), where about two paragraphs deal with antibiotics. On p. 311 about 5 paragraphs deal with antibiotics, but no mention is made of this under "antibiotics" in the index. Admittedly, working up an index to a book is a horrible job. But if it is not done properly, the book suffers correspondingly, and cannot attain its maximum usefulness.

WALTER C. TOBIE

CATGUT STERILIZATION. *The Basic Principles of the Spore-Germinating Method.*

By Henry L. Christensen. Nyt Nordisk Forlag, Arnold Busck, Copenhagen. Paper. 174 pp.; ill. 1944.

This is the English translation of a thesis submitted in 1943 for the M.D. degree at Copenhagen University. The author sterilizes catgut by repeatedly placing it in conditions where the spores of aerobic and anaerobic bacteria germinate. The vegetative cells are then killed with iodine. Excess iodine is neutralized with thiosulfate and the germination and sterilization are then repeated. Iodine is used for the final sterilization. Much careful experimental work is described. Literature on the preparation of surgical catgut is exhaustively reviewed and critically analysed. The excellent bibliography contains 230 references.

The author's process is probably not economically practicable for the commercial sterilization of catgut. Current American practice is based upon the heat-sterilization of well-dried catgut under carefully controlled conditions, although there may also be a pretreatment or posttreatment with germicidal or bacteriostatic agents. If properly processed, the product is invariably sterile.

Nevertheless, the book is a valuable contribution to the literature of a small, highly specialized field. It should be of great interest to those concerned with the manufacture of catgut and other types of surgical sutures.

WALTER C. TOBIE

ZINSSER'S TEXTBOOK OF BACTERIOLOGY. *The Application of Bacteriology and Immunology to the Diagnosis, Specific Therapy and Prevention of Infectious Diseases for Students and Practitioners of Medicine and Public Health. Ninth Edition.*

Revised by David T. Smith, Donald S. Martin, Norman F. Conant, Joseph W. Beard, Grant Taylor, Henry I. Kohn, and Mary A. Poston. Appleton-Century-Crofts, New York. $10.00. xlii + 992 pp.; ill. 1948.

The Duke University group listed above has extensively revised this ninth edition of a standard work. The book has been brought well up to date, and illustrates the progress made in bacteriology since the earlier editions, which some of us remember from long ago under the colloquial designation of "Hiss and Zinsser." The main stress is laid upon the bacteriology of pathogenic microorganisms, including (besides the bacteria proper) the spirochetes, the pleuropneumonia group, the rickettsiae, the filterable viruses, and the pathogenic fungi. Infection and immunity also receive a good deal of attention. The book is mainly descriptive, although the last 9 chapters are devoted to laboratory procedures. The subject matter is well correlated with the references wihich follow each chapter.

Despite a generally excellent job of revision, the book has certain weaknesses. The index lists *Streptomyces griseus* but does not mention streptomycin, although this antibiotic is discussed on p. 99 and elsewhere. An amusing misprint occurs on p. 554, where "hay fever" instead of "hay bacillus" is given as a synonym for *Bacillus subtilis*. We hope that no over-serious student will be led to regard *B. subtilis* as a pathogen.

Furthermore, the tables giving the properties of antibiotic substances (pp. 100–105) are loaded down with misinterpretations and errors. The filtrate misnamed "aspergillin" by Soltys (1944) is not the same thing as aspergillin, the black water-insoluble pigment of certain *Aspergillus* spores. Fleming's lysozyme (1922) is tabulated under "Antibiotics from higher plants," but with "animal tissues and egg white" listed as the source! Although Wrede and Rothhaas studied prodigiosin, they were not the ones who reported its alleged antibiotic properties in 1945. Furthermore, this pigment is *not* water soluble, as claimed in the tabulation.

When the tenth edition of this useful book appears, we hope that some of the weak spots in the current edition will have been cleared up.

WALTER C. TOBIE

HEALTH AND DISEASE

MICROBES MILITANT: A CHALLENGE TO MAN. *The Story of Modern Preventive Medicine and Control of Infectious Diseases. A Revision of "The Microbe's Challenge."*

By Frederick Eberson. The Ronald Press Company, New York. $4.50. xii + 401 pp. + 15 plates; text ill. 1948.

This volume (one of the Humanizing Science Series) is a revision of *The Microbe's Challenge*, published in 1941. It is written in an interesting style, and can be recommended to non-bacteriologists who want an up-to-date general picture of bacteriology, particularly pathogenic bacteriology. Some of the explanations of special topics, such as immunology and epidemiology, are very well done. Unfortunately, there are too many loose interpretations, even though downright errors seem to be rare. The tanning of leather (p. 12) is represented as being "a process of putrefaction (rotting)." This is roughly true of some methods of bating and dehairing leather, but is not true of tanning in the strict sense. It is misleading to say (p. 20) that "the botulinus germ can be kept at boiling temperatures for more than five hours without impairing its efficiency," although botulinus *spores* may resist prolonged boiling. The tubercle bacillus is undoubtedly rich in lipoids, but the notion of "a waxy coat of armor enveloping its body" (p. 176) is a somewhat outdated conception which has proven misleading. Of the metabolic products of *Pseudomonas aeruginosa* (*Bacillus pyocyaneus*) (p. 186), pyocyanase is not the same thing as pyocyanin. Minor flaws such as these detract from the value of the book, which is otherwise an excellent attempt to interpret bacteriology to the general reader.

WALTER C. TOBIE

MICROBIOLOGY AND PATHOLOGY. *Fourth Edition.*

By Charles F. Carter. The C. V. Mosby Company, St. Louis. $5.00. 845 pp. + 25 plates; text ill. 1948.

This is a textbook intended for the instruction of nurses (and perhaps medical laboratory technicians). As such, it does a sound, commendable job, both in microbiology and pathology. Tests for readers are given at the end of each chapter. Each chapter is also followed by a number of useful references. Some of the material may seem elementary to specialists, but it must be remembered that a wide knowledge, even if superficial, is necessary and helpful to most workers in the medical sciences.

One trifling error is the mention of Leeuwenhoek as a "wealthy man who devoted the major portion of his time to scientific studies." A much more serious failing lies in the variable printing. In the copy received by the reviewer, it varied from very pale to very dark, the latter approximating boldface type. A colleague versed in the printing arts attributed this to "bad make-ready," without further qualification. In any event, irregular printing is not desirable in any book. When it occurs in a book whose contents are mainly sound and useful, the results are doubly unfortunate.

WALTER C. TOBIE

MEDICAL MICROBIOLOGY AND HYGIENE. *Excerpta Medica, Section IV, Volume I, Number 1, January 1948. Edited by M. W. Woerdeman, A. P. H. A. De Kleyn, and W. P. C. Zeeman. Excerpta Medica, Amsterdam.* Sections I, IV, and VIII, each $22.50 or £5.12s per yearly volume of about 900 pages.

See comments under *Anatomy, Anthropology, Embryology and Histology*, Section I of *Excerpta Medica*, on p. 154 of this issue.

BOVINE MASTITIS. *A Symposium. McGraw-Hill Publications in the Agricultural Sciences. Edited by Ralph B. Little and Wayne N. Plastridge. McGraw-Hill Book Company, New York and London.* $7.00. xiv + 546 pp.; ill. 1946.

This collection of authoritative reviews brings up to date the work begun in the monumental review of the subject by E. Munch-Petersen, in 1934. The several contributions are as follows: Anatomy of the Udder (Mack A. Emmerson); Physiology of Milk Secretion (C. W. Turner); Pathology of Mastitis (Lloyd B. Sholl); Diagnosis of Mastitis. I. Examination of the Udder (Leland J. Tompkins), and II. Examination of Udder Secretion (Wayne N. Plastridge and Claude S. Bryan); Bacteriology of Mastitis (Ralph B. Little, J. Howard Brown, and Wayne N. Plastridge); Serological Classification of the Mastitis Streptococci (Arthur W. Stableforth); Environmental and Hereditary Factors (Little); Transmission (Little); Mastitis in Heifers (Oscar W. Schalm); Eradication of Chronic Mastitis (Plastridge); Vaccination in the Control of Bovine Mastitis (Schalm and Plastridge); Treatment of Bovine Mastitis (Schalm and Little); Public Health Significance of Bovine Mastitis (Brown); Relation of Bovine Mastitis to Quality of Dairy Products (Elmer O. Anderson); Stable Hygiene in the Control of Mastitis for the Production of Clean Milk (Bryan, Schalm, and Plastridge); Recommendations for State Control Programs (Schalm, Bryan, and Plastridge); Mastitis in Goats (Bryan). There is an appendix dealing with techniques used in testing for and in studying this disease.

Although the book will be of primary interest to those in the fields of animal husbandry and animal pathology, there is much material of general biological interest in it. The editors are to be commended for producing so well-rounded and complete a survey of the environmental and biological backgrounds of the disease. There are very few monographs of human diseases that approach it in this respect.

VIRAL AND RICKETTSIAL INFECTIONS OF MAN. *Edited by Thomas M. Rivers. J. B. Lippincott Company, Philadelphia, London, and Montreal.* $5.00. xviii + 587 pp. + 6 plates; text ill. 1948.

Four of the 37 chapters in this excellent book were contributed by the editor, and the remainder by 26 other workers. Limitations of space hence preclude a summary of individual chapters by authors. The work is not a monograph for specialists, but gives an overall picture of the subject without going too deeply into controversial topics. It is thus mainly a textbook for medical students and a reference work for physicians who do not specialize in viral and rickettsial diseases. Besides descriptions of the individual diseases, chapters are devoted to diagnostic laboratory and research procedures, such as serologic reactions, chick embryo technics, and tissue cultures. There is also a chapter on epidemiology. Each chapter is followed by a number of useful references. The author and subject indexes seem to be very complete. The book is well printed on good paper. Its price is much lower than would otherwise be the case, owing to a subsidy from the National Foundation for Infantile Paralysis. This foundation, the editor, and his collaborators, deserve great credit for the production of this highly valuable work.

WALTER C. TOBIE

THE ACUTE BACTERIAL DISEASES. *Their Diagnosis and Treatment. By Harry F. Dowling; with the collaboration of Lewis K. Sweet and Harold L. Hirsh. W. B. Saunders Company, Philadelphia and London.* $6.50. x + 465 pp.; ill. 1948.

This excellent book contains a considerable amount of new material, based on the author's observations of more than 7,000 cases. Part I (6 chapters) covers diagnosis and general measures; also serum, sulfonamide, penicillin, and streptomycin therapy. Part II (6 chapters) deals with infections caused by cocci (pneumococci, streptococci, staphylococci, meningococci, and gonococci). Part III (10 chapters) deals with infections by bacilli (*Eberthella, Shigella, Brucella, Mycobacterium tuberculosis* (acute infections), *Hemophilus pertussis, H. influenzae, Klebsiella pneumoniae, Pasteurella tularensis, P. pestis*, and *Bacillus anthracis*). Part IV (4 chapters and appendix) covers infections in which exotoxins are a major factor (diphtheria, tetanus, gas gangrene, and miscellaneous bacterial diseases). Lewis K. Sweet and Harold L. Hirsch assisted in several chapters. The book is written in a clear and interesting style, without any sacrifice of accuracy. Controversial topics are presented in a fair and well-balanced fashion. Each chapter is followed by a number of useful references. The book can be highly recommended.

WALTER C. TOBIE

THE INHIBITION OF MALARIAL RELAPSES BY TOXOID OF CLOSTRIDIUM TETANI. *Annals of the New York Academy of Sciences, Volume 50, Article 3, June 7, 1948.*

By Eusebio Y. Garcia. The New York Academy of Sciences, New York. 75 cents (paper). Pp. 171–186; ill. 1948.

In the Philippine Islands, 319 patients with *P. vivax* or *P. falciparum* malaria were treated with atabrine or chloroquine. When, in addition to this treatment, 1 cc. of tetanus toxoid was injected subcutaneously, with a second injection 21 days later, the average relapse rate and average time before relapse was improved from 90 per cent relapses after 115 days to 9.5 per cent relapses after 150–315 days. Unfortunately, it was not possible to observe the effects of tetanus toxoid in the absence of conventional anti-malarial treatment. Garcia considers that the toxoid acted non-specifically by stimulating the phagocytic activity of the reticulo-endothelial system. The work is therefore akin to the non-specific protein therapy in vogue about 20 years ago for bacterial (and supposedly bacterial) infections, as well as the much more recent work of Bogomolets, the Soviet investigator. A careful check on this work by other malariologists seems desirable.

WALTER C. TOBIE

THE ACUTE INFECTIOUS FEVERS. *An Introduction for Students and Practitioners.*

By Alexander Joe. The Blakiston Company, Philadelphia. $4.50. viii + 276 pp. + 21 plates; text ill. [No year].

This book was printed in Great Britain, apparently in 1948, although the date is not given. It is based on the author's experience in teaching the subject of acute fevers in London and Edinburgh. Chapters are devoted to scarlet fever, erysipelas, puerperal sepsis, diphtheria, whooping-cough, cerebro-spinal fever, enteric (typhoid and paratyphoid) fever, measles, chickenpox, smallpox, vaccination, mumps, rubella, erythema infectiosum, and serum reactions. Other fevers receive little if any mention. There is no bibliography, although authors' names (with dates) are mentioned freely in the text. The main emphasis is clinical. American clinicians (and to a lesser extent bacteriologists) will find the work of interest chiefly for the light which it casts on current (or recent) British medical practice. In conditions where the sulfa drugs are useful, the main emphasis seems to be placed on the use of the older sulfonamides: sulfanilamide, sulfapyridine, and sulfathiazole. The newer sulfonamides receive but scant mention. On p. 149, under cerebro-spinal fever (cerebro-spinal meningitis) the author remarks that in his experience sulfadiazine and sulfamethazine are no more effective than the older drugs. Penicillin generally receives favorable mention.

WALTER C. TOBIE

PUBLIC HEALTH ENGINEERING. *A Textbook of the Principles of Environmental Sanitation. Volume I.*

By Earle B. Phelps and collaborating authors; Part One with a Chapter on Insects and Insect Control by Harry D. Pratt; Part Two in collaboration with Clarance J. Velz. John Wiley & Sons, New York; Chapman & Hall, London. $7.50. x + 655 pp.; ill. 1948.

Volume I of this text contains a tremendous amount of useful and interesting material, and it is regrettable that Volume II was not made available at the same time. Volume I is divided into two parts: Part 1, the sanitation of air; Part 2, the sanitation of water. The book is written primarily for the engineer who knows *how* to design and build, and provides further information on *what* to design and build, giving the reasons in the light of present knowledge of physiology, bacteriology, epidemiology, and other biological sciences. Lying in a field halfway between biology and engineering, it is difficult to characterize the book for biologists in a limited space, except to say that it is an outstanding work in its field. As a text in sanitary engineering at an advanced level, it can be highly recommended. The sole defect noticed was a rather weak index, making it necessary to do a good deal of digging around in the text in running down some special topic.

WALTER C. TOBIE

HISTOPATHOLOGY OF IRRADIATION FROM EXTERNAL AND INTERNAL SOURCES. *National Nuclear Energy Series; Manhattan Project Technical Section. Division IV—Plutonium Project Record, Volume 22 I.*

Edited by William Bloom. McGraw-Hill Book Company, New York, Toronto, and London. $8.00. xxvi + 808 pp. + 6 plates; text ill. 1948.

Reports of the various phases of the research performed under the now famous Manhattan Project have been in preparation since 1944 as the National Nuclear Energy Series (Manhattan Project Technical Section). The declassified portion of this series is expected to comprise a group of about 60 volumes in eight divisions, of which the report under consideration is volume 22 I of Division IV (Plutonium Project). The Plutonium Project Record covers most of the work whose initial objectives were the development of chain-reacting piles to produce plutonium and the development of fission bombs. It was realized that the liberation of nuclear energy would release large amounts of biologically harmful radiation, making it imperative to establish "tolerance levels" for personnel and to anticipate by histological study the effects of explosive liberation of such energy on military and civilian populations. William Bloom was chosen to head the Histological Group of the Health Division. He has presented and summarized the reports of the eleven contributing authors in this highly significant volume.

The experimental design of the work was not that which would have been chosen by trained scientists under ordinary circumstances. Procedure was everywhere determined by the urgent need to discover the lethal dose for the various types of radiation, applied externally or internally, and to discover the effects on animals sacrificed at various times after treatment with one or more applications. X-rays, the effects of which had already been extensively investigated, were used as a base line for comparison with other types of radiant energy. External irradiation was produced by x-rays, beta rays, gamma rays, fast neutrons, and slow neutrons. Internal irradiation was produced by the radioactive isotopes of a dozen or so elements which emitted alpha, beta, and gamma rays. These isotopes were administered intramuscularly, intravenously, intracardially, intraperitoneally, by inhalation, and by gavage.

The work had to be done hurriedly. Often examinations were briefer than was satisfying to the workers. The present report is incomplete, in that all of the experimental animals in some of the series had not been sacrificed at the time of writing. Nevertheless, the investigations have been highly productive. They are reported under organological and tissue group headings which are illustrated by 16 full-page diagrams, 24 camera lucida drawings (mostly in color) and more than five hundred photographs. Although the field is generally interpreted to be a new one, references in the text to previous work are copious and a bibliography of 237 items is included. An appendix gives detailed experimental data. As in any book written by different authors, the style and clarity are irregular. However, the entire series has been summarized in a final chapter by Bloom himself, whose analysis of the material testifies to the homogeneity of the results and presents a clear picture of the whole contribution.

It appears that specific effects are not caused by radiant energy from the different chemical elements, isotopes, etc., which were used, with the possible exception of that from x-rays acting on bone metaphyses, where secondary rays may be responsible for the greater effectiveness. When adjusted to equally lethal levels, the various sources of radiant energy cannot be differentiated according to their effects in a given species. Radioactive isotopes have the same effect as externally applied irradiations, but are subject to conditioning factors such as amount, mode of introduction, diffuse or localized distribution, rate of elimination, rate of decay, and types and energy of emitted particles. Radiosensitivity and radioresistance can be classified according to species, individuals of a species, organs, and cells. Among guinea pigs, rats, and rabbits, the guinea pigs are the most sensitive and rabbits the least so. Young animals appear to be more sensitive than adults. Among the organs, those with lymphatic tissue and the testis are most susceptible to damage. The ovary, bone marrow, skin, and intestinal crypt epithelium, among others, are somewhat more resistant. The liver, adrenal, lung, kidney, pancreas, and nervous system are highly resistant. The resistance of the kidney is notable because of the presence of localized radioactive isotopes. Among cell types it appears that the environment is a conditioning factor. Lymphocytes are highly sensitive, but this depends somewhat on the organ in which they are located. Spermatogonia are highly sensitive, sharing top susceptibility with the lymphocytes, but spermatocytes, spermia, spermatids, Sertoli cells, and interstitial cells are resistant. The changes in germ cells are carried to the next generation. Among other susceptible cells are erythroblasts, myelocytes, megakaryocytes, osteoblasts, articular cartilage cells, and the cells responsible for regeneration in the gastro-intestinal epithelium. Among resistant cells are reticular cells and free macrophages. Cytologically, the nucleus is the indicator of cell damage, the cytoplasm not being as responsive. The nuclear changes detected in early damage are probably reversible and are not specific to irradiation. The collagenous fibers of connective tissue are susceptible to damage. The previously current ideas that primitiveness of cells causes sensitivity and that cells preparing for or in mitotic activity are highly susceptible are not supported and ought be discarded. There is no evidence of stimulation by small dosage. The possibility that general tissue damage may result from toxins liberated by irradiated protoplasm is considered, the generalized reaction in cases of local irradiation supporting this.

Although much information of great value has been obtained, it is frankly stated that the fundamentals of damage by irradiation are still not understood. No information on the basic mechanism is available. The patterns according to which some tissues and cell types are affected and others are spared are well recognized, but the reasons for this selectivity are unknown. It is not known why irradiated animals die.

This report represents only a small part of the greatest technological effort in the history of mankind, but it has nevertheless made contributions of great potential interest to biology and medicine. An interesting and challenging field has been largely created, extensively studied, and neatly reported, all within the scope of this single volume. Its significance cannot be overlooked. Bloom and his associates deserve great praise for their work.

F. N. Low

CLINICAL AND GENETIC STUDIES ON THE PRIMARY GLAUCOMA DISEASES. *Opera ex Domo Biologiae Hereditariae Humanae Universitatis Hafniensis. Volume 12.*

By Erik Westerlund. Nyt Nordisk Forlag, Arnold Busck; Ejnar Munksgaard, Copenhagen. Kr. 12.00 (paper). 207 pp.; ill. 1947.

The results of a statistical survey of primary glaucomatous conditions from Danish hospitals of the past 50 years is reported in this dissertation. The material is analysed in 17 chapters discussing genealogic evidence, correlations with ocular and other defects, uni- and bilaterality, and incidence, with special reference to sex, age, social environment, and geographic distribution. It is arranged according to the following classification: 1) congenital or infantile glaucoma (hydrophthalmos); 2) glaucoma in adults, with subdivisions a) simple glaucoma, and b) intermittent glaucoma. More than one-half of the dissertation deals with congenital hydrophthalmos, of which 31 familial cases and 91 solitary cases are reviewed. In the author's opinion, the consideration of hydrophthalmos as a developmental anomaly would give us "an understanding of the familial occurrence of the disease on a hereditary basis"; this viewpoint is open to doubt (see, e.g., malformations of the eye and other organs due to German measles of the pregnant mother!). The bulk of the evidence indicates a recessive inheritance in hydrophthalmos. Weinberg's sibling method reveals manifestation in sibships in only 9.8 per cent, i.e., only 40 per cent of the 25 per cent theoretically expected. The author stresses justly that neither hydrophthalmos nor primary glaucoma in adults is likely to be a nosologic entity. Evidence from the correlation of age incidence curves suggests that hereditary glaucoma in adults manifests itself chiefly as an intermittent glaucoma, while by the same token simple and hereditary glaucoma differ from each other. For the intermittent type, a dominant mode of inheritance with incomplete penetrance emerges from the genealogic and sibship analysis. The author's implication that hereditary glaucoma manifests itself chiefly as intermittent glaucoma may, however, be questioned: while the number of his hereditary cases was much smaller in the simple type group than in the intermittent one, the obstacles to an adequate sampling of affected relatives are greater in the simple type of glaucoma, both because of the more insidious character of the manifestations, and because of its relatively greater incidence at higher age.

The lack of correlation between the inheritance of glaucoma and the depth of the anterior chamber, the problem of anticipation, and the reasons for considering congenital and adult glaucoma as genotypically different diseases are discussed, and a chapter on eugenics is appended. A particular asset of the work is the stress laid on the importance of environmental factors which precipitate the manifestation of the disease in many cases.

In appraising the value of this work, one has to bear in mind the still existing difficulties in classifying glaucomatous conditions from a nosologic standpoint. The application of refined physiologic methods of analysis of neuro-vegetative regulations and fluid circulation in the eye (e.g., pupillography, observation of aqueous veins, function tests) may eventually point the way to a classification according to pathogenetic rather than descriptive principles. The importance of this trend for the genetic approach, e.g., for the question of variable gene expressivity, becomes apparent in the work of Posner and Schlossman on the inheritance of glaucoma (*Trans. 52nd Ann. Sess. Amer. Acad. Ophthal. & Otol., Oct., 1947:* 145–159, 1948). Westerlund follows an older, grossly descriptive clinical approach. Nevertheless, since due precautions in statistical methods and interpretations have been carefully applied in this work, it will be valued as an important contribution to the subject.

WILLIAM BUSCHKE

DEMENTIA PRAECOX. *The Past Decade's Work and Present Status: A Review and Evaluation.*

By Leopold Bellak, with foreword by Winfred Overholser. Grune & Stratton, New York. $10.00. xvi + 456 pp. 1948.

Here is an excellent summary of virtually all that has been written of any significance on the topic of schizophrenia. There are some twenty chapters covering the definition and description of the disease, its vital statistics, etiology, pathogenesis and pathology, diagnosis and symptomatology, physiological studies, psychological studies, general aspects of therapy, shock treatments of one sort or another, other types of somatic therapy, psychotherapy, prognosis, complications and sequelae, prevention, childhood dementia praecox, medicolegal aspects, and schizophrenia in military life.

The conclusions are conservative and ably presented. The author concludes that schizophrenia may result from a variety of etiological factors and may range from being theoretically completely psychogenic in nature to being completely organic. He chooses to call this a psychosomatic view of schizophrenia. He singles out for special note a type occurring in people with adequate prepsychotic structure, with sudden onset, presence of precipitating factors, confusion with catatonic symptoms, and some affective features. This type has an unusually good prognosis.

Bellak calls for more and better coordinated research into the illness, and for a standardization of diagnostic criteria and of objective criteria for clinical status and the results of treatment. He suggests a change in diagnostic categories, and divides them roughly into three sorts: 1) schizophrenia of a predominantly psychogenic nature and a more often benign course; 2) dementia praecox, possibly more constitutionally determined; and 3) schizophrenoid psychosis, a particularly brief and benign disorder with schizophrenic symptomatology and with a dreamlike confused state and hysteria-like symptoms, as noted above.

The book is outstanding for its thorough combing of the literature, both domestic and foreign, and will

be from this standpoint alone very useful to everyone interested in the topic.

WENDELL MUNCIE

METHODS IN MEDICAL RESEARCH. *Volume I. Edited by Van R. Potter, Henry Welch, Harold D. Green, and A. C. Ivy. The Year Book Publishers, Chicago.* $8.00. xiv + 372 pp.; ill. 1948.

This is the first volume of a series to be devoted to methods and techniques of investigation. As pointed out in the preface, such a series may be useful and necessary to appraise, publish, and discuss the various scattered methods devised for use in any given field. Another function will be to gather together the many modifications of methods and also unearth useful methods that may have become buried in official reports during the war or otherwise.

It was planned that each volume would be divided into four or five sections, each representing one of the broad fields of medical research, such as biochemistry, physiology, pharmacology, microbiology, immunology, and biophysics, including radiobiology. Experts in the various fields will be chosen to act as associate editors to select the methods most worthy of being described and the contributors best fitted to describe them. The general usefulness of the series will depend on the care used in selecting the contributors and associate editors, and will not become well established until additional volumes have been added.

If the standard set by this first volume is an indication, the series should become quite popular, not only in fields of so-called medical research but in many others as well. Often the solution of a technical problem in one field can be applied to similar problems in other fields.

The first section (66 pages), on the methods used to assay antibiotics, and edited by Henry Welch, is timely and very well treated. One is amazed at the number of methods described for assaying antibiotics. It is stated by the editor of this section that not all of the reliable methods were included, but that the ones described in this volume had been given extensive trial and had proved satisfactory.

The second section is on Circulation, Blood Flow Measurement, and is relatively long (188 pages). The associate editor of this section invited a large number of investigators to describe the well known methods they have used or developed. The recently developed methods applicable to unanesthetized men and animals are especially interesting. For example, Warren's description of the technique of determining the cardiac output by Cournand's method of catheterization of the right auricle or ventricle of unanesthetized human subjects by introducing the catheter through an arm vein is indeed impressive. The description of the extension of this technique to the catheterization of the inferior vena cava to measure hepatic blood flow, by Stanley E. Bradley, who has accumulated considerable experience with the method, also deserves favorable comment.

The section on Selected Methods in Gastroenterologic Research, edited by A. C. Ivy, consists of two papers on subjects of current interest and deals with techniques used extensively by the authors. Van R. Potter, editor-in-chief of this volume, has selected an excellent group of contributors to the last section on techniques used in studies of cellular respiration. He has also contributed an excellent chapter on homogenate techniques. The book contains many interesting and valuable details that are usually omitted from short descriptions of techniques. The critical comments of other collaborators are also valuable.

FRANK H. J. FIGGE

INTERESTING AND USEFUL MEDICAL STATISTICS. *Edited by William H. Kupper. Wm. C. Brown Company, Dubuque, Iowa.* $6.50. viii + 528 pp. 1948.

This book is a collection of tables "after a study of many thousands of tables appearing in the principal U. S. journals during the past 10 years." The tables, many statistical, cover over 200 subjects and the author has scrupulously attempted to omit from his work unclear statistics. He also states that "unfortunately, the author cannot thank the editors of some journals who put impossible obstacles in the way of the accomplishment of his task, by requiring author's permission, journal editors' permission, and publishing company's permission, sometimes for the use of a single, small, statistical table!" Editors can be very annoying.

DAVID B. TYLER

PSYCHOLOGY AND ANIMAL BEHAVIOR

THE WORKS OF THE MIND. *By Mortimer J. Adler, Heinrich Brüning, Marc Chagall, S. Chandrasekhar, Alfeo Faggi, J. W. Fulbright, Robert M. Hutchins, C. H. McIlwain, John Von Neumann, Arnold Schoenberg, Yves R. Simon, Frank Lloyd Wright; edited for the Committee on Social Thought by Robert B. Heywood, with a Preface by John U. Nef. The University of Chicago Press, Chicago.* $4.00. xii + 246 pp. 1947.

This volume includes a series of papers that were given as lectures at the University of Chicago during the late winter and spring of 1946. It is an attempt to encompass the creative process in the arts, sciences, and in public life, and is an amazing group of documents by outstanding artists, public figures, scientists, and scholars. Each man has attempted to delimit his field and

to express his own conception of the working processes within it, the motives involved, and the end results.

It is impossible to sum up such a work briefly. Suffice it to say that within these pages are presented some of the most challenging conceptions involving human creative activity one is liable to meet anywhere. It seems to the reviewer that the artists present a less intelligible account of their creative processes than the scientists and those dealing with public affairs, or the scholars in history and philosophy, have done. Nevertheless, everyone seems to agree with Schoenberg that creative activities are achieved by the head and the heart together. Every one of the speakers has emphasized the motives at work within his field, whether it be for the production of esthetic pleasure, in the arts, for a kind of scientific pleasure in scientific orderliness, for the search for historical truth, or in philosophic judgment.

In the Preface by John U. Nef, Chairman of the Committee on Social Thought, which promoted the lectures, there is an appeal for a tempo in which works of the mind can develop unhurried.

There may not be many people who can actually read this book in its entirety, but those who attempt it will find themselves in the company of that group for which cultural understanding and a striving for some unity of purpose in the world is the all-important thing in life.

One finds among these contributions some exceedingly depressing commentaries on the status of our times, but one also finds here some of the most provocative challenges to that better understanding which is necessary for a better world.

In short, this is an extraordinary group of papers—every one of them scholarly in its own right. It deserves the most careful reading and consideration.

WENDELL MUNCIE

THE THIRD MENTAL MEASUREMENTS YEARBOOK. *Edited by Oscar Krisen Buros. Rutgers University Press, New Brunswick, New Jersey.* $12.50. xvi + 1047 pp. 1949.

In the last 40 years mental testing has almost become an industry. Sprinkled throughout the country are literally hundreds of testing centers, guidance centers, vocational advisory bureaus, and the like, whose stock-in-trade consists of tests to assess a customer's abilities, capacities, achievement, attitudes, character, and personality. Published tests of various sorts are also used widely by teachers, individual psychologists, and personnel managers because of their convenience and proven value. To supply this market, it has long been fashionable, and in many cases lucrative, to devise or invent new tests. Thousands are now available to do almost any kind of job—to detect color-blindness; to measure student achievement in specific academic subjects, e.g., biology, chemistry, science; to assess an individual's intellectual capacity; or to find out if the examinee is neurotic—just to mention a few. Some of these tests are excellent, the great majority are good, and many are poor. This diversity makes it essential that there be available an impartial, critical, and complete catalog of tests and of books on testing. The *Mental Measurements Yearbooks* are intended to be just such compendia.

The Third Mental Measurements Yearbook covers thoroughly the period from 1940—when the last Yearbook was published—through 1947. Many items and bibliographic references from earlier periods are included, however. The major portion of the Yearbook consists of two sections headed "Tests and Reviews" and "Books and Reviews." In addition, there are five indexes and directories: Periodical Directory and Index, Publishers Directory and Index, Index of Titles, Index of Names, and Classified Index of Tests.

The section "Tests and Reviews" lists 663 tests, 713 original reviews by 320 reviewers, 66 review excerpts, and 3,368 references on the construction, validity, use, and limitations of specific tests. This section lists all commercially available tests—educational, psychological, and vocational—published in English-speaking countries between October 1940 and December 1947. Also included are a number of tests published prior to 1940. For each test, the following information is given: title; description of the groups for which the test is intended; the date of copyright or publication; whether the test is machine-scorable; whether it is an individual or group test; the number of forms, parts, or levels available; the cost of the test; time needed to complete the test; the author and publisher. Following each test entry is a list of all known references, published and unpublished, on the construction, validity, use, and limitations of the test. Each reference is cross-referenced to abstracts and listings in *Psychological Abstracts*.

Most of the test entries are followed by test reviews. The majority of these were written specifically for this Yearbook at the invitation of the editor. They were prepared by specialists in the field and are, for the most part, critical and highly informative. Many reviews are excerpted or republished from reviews which appeared in other journals or books.

The section "Books and Reviews" lists 549 books on tests and closely related topics and 785 excerpts from reviews of these books published elsewhere. The list includes all books published in this field between October 1940 and December 1947 plus a few books, published earlier, which had not been listed or reviewed in preceding yearbooks.

The various indexes make it possible for the reader to locate readily all tests in certain classes, authors of tests, books and reviews, publishers, review journals, and listings of all of these in other sources.

With no further ado, the reviewer can say unquali-

fiedly that this is an extremely valuable reference volume for anyone who works with mental tests.

A. CHAPANIS

PSYCHOLOGICAL MONOGRAPHS. *Sex Differences in Dispersion at the High School and College Levels. The American Psychological Association, Volume 61, Number 1, Whole Number 282.*

By Jesse B. Rhinehart; edited by John F. Dashiell. The American Psychological Association, Washington, D. C. 75 cents (paper). viii + 37 pp. 1947.

This study of absolute and relative variability in scores on achievement and intelligence tests reveals little indication that either males or females of the United States at high school and college levels are more variable than or superior to the other sex. Males excel somewhat in mathematics and science tests and females excel on purely verbal tests, literature, fine arts, etc. In spite of differential selectivity in the educational program and in spite of differences in objectives and training, the statistical differences between the sexes, as measured by these tests, appear to be negligible.

PROBLEMS OF PHYSIOLOGICAL PSYCHOLOGY.

By J. R. Kantor. Principia Press, Bloomington, Indiana. $5.00. xviii + 398 pp. 1947.

In this textbook, the author describes the historical plight of physiological psychology in reaching any rational basis for its existence. He shows the errors involved in the traditional dichotomy of viewpoint about physiological psychology that either regards a) organic actions themselves as accompaniments of or explanatory mechanisms for psychic states, or b) holds that psychological events are sufficiently described as physiological actions of one sort or another. He comes to the conclusion that "basic to authentic physiological psychology is the proposition that physiological or biological actions constitute participating factors in larger interactional systems." He further shows that "the participative principle is equally applicable when 1) isolated responses or 2) comprehensive adjustments are considered." Actually, this is a brief for the author's interbehavioral psychology and why he continues to use the term "physiological psychology" it is hard to see. Why not speak of interbehavioral psychology instead of "an authentic physiological psychology"?

The chapter headings indicate the substance of the book. Section I is devoted to the historical background of physiological psychology, and describes its problems and paradoxes, its philosophical and scientific backgrounds, its British and Continental traditions, the historical sum and division of physiological psychology, philosophy, and psychological science, the brain in the history of science, and the biology and psychology of the nervous system. Section II gives an analytic survey of the data and constructions of physiological psychology, with special chapters devoted to general physiological psychology, sensory physiopsychology, psychophysiology, psychoneurology, experimental psychoneurology, clinical psychosurgery, psychochemistry, psychobiology, and psychosomatic medicine. Section III is devoted to suggestions for the emendation of physiological psychology.

As a critical statement of the philosophical and scientific fallacies of the past and present and of the search for philosophical and scientific validity in the present, this is a challenging book. One might hope that all the various schools that deal with organismal behavior might agree upon a common name and a common expression of philosophical and scientific principles. Kantor would certainly be represented in such a movement.

Was this book really intended as a textbook of physiological psychology? It seems more a statement made with the hope of clarifying the muddled present state of that science and of setting the stage for straight thinking on the subject.

WENDELL MUNCIE

PHILOSOPHICAL PSYCHOLOGY: *With Related Readings. A Text for Undergraduates. Revised.*

By Raymond J. Anable. The Declan X. McMullen Company, New York. $3.25. xxii + 361 pp. 1947.

In reading this book, your reviewer could not escape the feeling that he had suddenly been transported, by means of some fantastic time-machine, to the sixteenth century. It is difficult to believe that anyone today could write a book on any kind of psychology without making use of the results of at least one experiment. It would take an extraordinary kind of mentality to write such a book, and the author of this one does not quite qualify. He does refer casually to the "all or none law" (in a footnote), to Weber's Law, and to some experiments Koehler did with chimpanzees (also in a footnote). The thing that really disqualifies the author, however, is that he actually describes some observations made by Fabre on the hunting wasp.

Life, of course, is so much simpler if one doesn't do experiments. Thus, it is immediately apparent to anyone who has never done an experiment that

THESIS VII

Brute animals are not endowed with intelligence

PROOF

Intelligent beings: (1) know the *universal* and *immaterial*; (2) know immediate analytic principles, v.g., the principle of cause and effect; (3) manifest progress in their *knowledge* of *language* and *science* and in their *operations* (art) by *inventions* and skills of various kinds.

But, the brute animal gives positive evidence of the lack of all these things.

Therefore, the brute animal is not intelligent.

Simple, isn't it?

Furthermore, "the human soul is a substantial principle." The proof goes like this: "That which is the subject of accidental modifications must be a substantial principle. But, the human soul is the subject of accidental modifications. Therefore, the human soul is a substantial principle." In a similar vein, it is possible to show that there is nothing to evolution, and many other wonderful things.

Teen-agers have an expression for this kind of nonsense. They call it "double-talk."

A. CHAPANIS

THE CLINICAL APPLICATION OF PSYCHOLOGICAL TESTS: *Diagnostic Summaries and Case Studies. The Menninger Foundation Monograph Series Number 6.*

By Roy Schafer; foreword by David Rapaport. International Universities Press, New York. $6.75. 346 pp. 1948.

In 1946 Rapaport, in collaboration with Gill and Schafer, published a two-volume treatise called *Diagnostic Psychological Testing*. In this work the authors attempted to provide well worked out descriptions, explanations, and rationales for some of the more frequently used diagnostic tests in the modern clinic. A major criticism of the work resulted from the fact that the volumes could not be effectively used by unexperienced clinical psychologists, who require some sort of guide to the use of these complex instruments in differential diagnosis and personality interpretation. Schafer has now provided the clinician with such a manual, and it should be a welcome beginning for anyone interested in determining how the experienced clinician goes about his diagnostic job.

The chief aim of the book is to describe accurately the operations by which the interpretative conclusions made by the clinician from diagnostic tests are evolved. One is certain that the complex job of evaluating and validating what has been conceded to be essentially an art cannot progress without such a clear statement of these techniques and principles. The difficulties underlying such a task as Schafer has undertaken are extreme. While the book does not represent a final achievement in this direction, it does succeed in being a first and most essential step in the right direction.

There were many ways in which the author might have attacked the problem, as Schafer himself points out. He lays considerable emphasis on diagnostic categories. The tests included are of course not all of those that are available for clinical use. The theoretical orientation is psychoanalytic. The ticklish question of validation is not dealt with. The number of cases in the sample on which the interpretative conclusions are based is severely limited, and there are no really original or overwhelming ideas presented. These are not serious criticisms, for within the scope and frame of reference of the book, Schafer has written an exceedingly clear and instructive clinical manual.

Following a short chapter on general considerations, there comes the most important section of the volume, dealing with a series of diagnostic summaries of most of the major clinical syndromes. Each diagnostic entity is described, followed by the characteristic test picture to be found on the Wechsler-Bellevue Scale, The Babcock test of learning efficiency, The Sorting Test, The Rorschach Test, The Word Association Test, and the Thematic Apperception Test. The next chapter provides excellent and complete case studies exemplifying each of the syndromes, including the complete test protocols, their analyses, and a short clinical history. A fourth chapter includes some briefer case studies. Schafer concludes with some remarks pertinent to the problems in the way of the realization of the aims of his book.

However one might disagree with the principles and generalizations made by a practising clinical psychologist with their limitations, it is hardly possible to say of Schafer's presentation that what he does in case evaluation is not clear. This is his major contribution, that he has provided a clear statement of his methods. What he does is therefore subject to repetition and verification—the two most essential requirements of any scientific procedure.

RICHARD S. LAZARUS

THE PROCEEDINGS OF THE THIRD PSYCHOTHERAPY COUNCIL, *October 18 and 19, 1946.*

Institute for Psychoanalysis, Chicago. $2.00 (paper). 176 pp. [No year].

This, the third volume in a series which used to be called *Proceedings of the Council For Brief Psychotherapy*, turns out to be an exceedingly interesting volume and a fitting successor to the two earlier volumes of the series. After a general introduction by Franz Alexander, the contents of the book divide into a formal presentation and discussion of three topics: 1) a case of peptic ulcer and personality disorder, by Franz Alexander; 2) a case of migraine, by Adelaide M. Johnson; and 3) the psychiatric out-patient clinic of the future: a symposium, by Roy R. Grinker. There is an impressive list of discussants who represented various schools of thought and even sub-schools within the psychoanalytic movement itself.

While the word "brief" has been dropped from the title of the *Proceedings*, the *Proceedings* essentially have to do with brief treatment of psychosomatic disorders involving the personality difficulties (in so far as the first two contributions are concerned). In both Alexander's and Johnson's cases, a very desirable thera-

peutic result was obtained in the course of what would ordinarily be considered a short time—a few months only, with a total of very few interviews. The opinions of the discussants were somewhat divided as to whether the methods used could properly be called psychoanalytic, and whether the physician concerned had any right to discontinue treatment without having gone through all the stages of a classical Freudian analysis of character disorder. Most eclectic-minded physicians will applaud the attitude, represented by Alexander, Johnson, and French, that, in fact, what the patient came for was demonstrably alleviated not only symptomatically but basically, and following Freud's own advice, they had no business bringing up potential material of no direct pertinence to the situation.

To the reviewer, who has had similar treatment experiences without the comfort (?) to be derived from an analytic formulation of procedure, it has seemed that Alexander and Johnson have done what he and many with the same outlook have also done, namely, they have thrown as much of the burden of treatment as the patient could bear on the patient himself, and have made the number of visits, for instance, parallel closely the need for dependent support and help. It is a hopeful sign in psychiatry to see addicts to any strict therapeutic method writhe at the success of unorthodox methods. This kind of thing will certainly in time promote that common understanding which some day will no longer be labeled "psychoanalytic" or any other term but will be a matter of commonsense understanding.

It seems to me that these conferences would be still more effective if they were to include either formal presentations, or at least discussions, by physicians non-psychoanalytically oriented yet able with some reason to claim to be dynamically oriented. This would keep the conferences from seeming to be psychoanalytic affairs and make them vehicles of broad understanding and collaboration. Or would this constitute a brand of heresy which would be intolerable to Alexander and his associates?

Anyway, these two cases presentations are exceedingly interesting and show great skill on the part of the therapists in choice of patient and in therapeutic method, and must give a glow of satisfaction to many who have long been convinced that methods for treating more people must be devised.

The third topic, which deals with the out-patient clinic of the future, is less stimulating than the first two, on the whole, but is of vast import for the future development of psychiatry. Carl Binger's remarks about the appropriate place in the hospital set-up for the out-patient department merit very careful attention.

On every count this booklet deserves to have a wide distribution and to be read carefully. It is to be hoped that such conferences will continue. Their substance could very profitably be made a matter for discussion in various centers about the country.

WENDELL MUNCIE

PSYCHOLOGY APPLIED TO NURSING. *Third Edition.*
By Lawrence Augustus Averill and Florence C. Kempf. W. B. Saunders Company, Philadelphia and London. $2.50. xvi + 496 pp.; ill. 1947.

AN OUTLINE FOR TEACHERS. *For use with the Third Edition of Psychology Applied to Nursing.*
By Lawrence A. Averill and Florence C. Kempf. W. B. Saunders Company, Philadelphia and London. Paper. ii + 102 pp. 1946.

This third edition is a revision of a textbook originally published in 1938 which has been in considerable use in the teaching of psychiatric nursing. It is no doubt indispensable in schools of nursing where it is the chosen book and where the teachers can found their courses upon the material contained in it; but to use it in another school or along with other books would be exceedingly difficult. The suggested exercises and reading are far more than the ordinary nursing student can attempt with the time at her disposal and would conflict greatly with any curriculum which was not absolutely based on the book. The book is well written, clear in its diagrams and explanations, sufficiently simple and lucid enough to be a first class textbook.

DAVID ROSS

APHASIA: *A Guide to Retraining.*
By Captain Louis Granich; appendix in collaboration with Sergeant George W. Pangle. Grune & Stratton, New York. $2.75. vi + 108 pp. 1947.

This volume is very largely based on work done in the World War II with aphasic patients in military hospitals. Much information of great psychological and pedagogical value was obtained from this work. The most significant part of the book is a detailed account of the retraining of eight patients. An important contribution made by the author is the simplification of terminology. Granich has broken down the difficult speech disabilities into specific fields, such as dysarthria, motor aphasia, receptive aphasia (auditory and sensory), reading disabilities (alexia and related disorders), etc. A list of abbreviations presented by Granich is of great help in dealing with the material, saving a lot of space and supplementary explanation. For example, motor aphasia is abbreviated to MTA, dysarthria becomes DTR, and failure to revisualize the word is presented as FRW. Granich describes in full his clinical approach, which includes such procedures as having a patient write a simple sentence from dictation, measurement of visual-motor responses, and the use of

a chart with letters of the alphabet for help in reading. If the speech defect is severe, the patient imitates the words and sounds; throughout the program the compensatory methods being used by the patient are sought.

As for general principles of retraining, these are very few. There are all sorts of approaches, and no particular one will fit in every situation. One important point the author makes is that we are forced to avoid rituals. These always present a tempting technique, as in making an appeal to the five senses. A patient who cannot recall the word "apple" may be shown an apple, required to smell it, feel it, and taste it. However, he still remains impotent to find the vocal symbol for the object, and a stimulus must be found to evoke the word. Likewise with group lessons, group recitations, group chanting, and so on, Granich points out that these are likely to turn out to be no more than ceremonies. He also emphasizes the importance and desirability of psychotherapy to accompany the specific training work.

The third chapter deals with techniques and is bound to be of tremendous interest to anyone engaged in actual remedial work with aphasics. No detail is too trivial for Granich to consider. As a manual or training guide this chapter will be of particular utility.

Chapter IV goes on to discuss the general defects in brain injury. Granich points out here that aphasic symptoms do not exist in isolation but are often accompanied by personality changes, mental deterioration, or physical handicaps. Neurological changes, such as epilepsy, paralyses, and vasomotor disturbances are also considered. Finally, considerable attention is given to post-traumatic neuroses and psychotherapy for these. Revelation of anxiety in Rorschach responses of these patients, as manifested by vague shading responses, and control of the anxiety by vista responses, are pointed out. In general, the Rorschach test appears to be a most useful diagnostic tool. The digit span test is also a good detector of anxiety. The author also refers to the work of Goldstein and Scheerer in pointing out perseveration as a revealer of general mental deterioration. Fatigability, "loss of the abstract attitude," and other elements are also significant in this area.

In the appendix, which is also written by Granich, with the assistance of Sgt. George W. Pangle, the actual work that was done in the retraining of the eight selected cases in an army general hospital is described in detail, case by case, pointing out the symptoms, source of the trauma, and the actual remedial procedure. Following each case there is a discussion of the general conclusions which might be drawn from the work with that particular case. In many cases, psychological and psychometric findings are reported, such as those of the Wechsler Memory Scale, Benton Visual Retention Test, and a number of others.

There is a bibliography of thirty-three titles, the type face is large, and the book is of a handy size. The book is recommended unhesitatingly as a manual for anyone engaged in work in this field, or in any work with disabilities in speech, reading, writing, spelling, etc. It should also be useful as a reference book in any work with aphasia, in psychiatry or neurology.

ARTHUR LICHTENSTEIN

THE REACH OF THE MIND.

By J. B. Rhine. William Sloane Associates, New York. $3.50. viii + 234 pp. + 8 plates. 1947.

This is a book on the subject of "extra-sensory perception" and "psychokinetic forces" which the intelligent layman can understand and from which he can probably benefit. A short historical review of psychical research is given, and this is followed by a discussion of telepathy and its experimental proof. Rhine then discusses clairvoyance, which he defines as "the awareness of objects or objective events without the use of the senses" and differentiates from telepathy. He groups those two abilities under the name of extra-sensory perception. He proceeds to discuss the experimental work and to show that statistically a subject can exceed the scores expected on the basis of chance as a mathematical probability and therefore one may postulate this extra-sensory ability. He finds this faculty to be independent of both distance and time.

Although one's powers of credulity are considerably stretched in considering the author's work on "psychokinesis," which is defined as "the ability of the mind directly to influence the motion of material objects," he gives even here a lucid and convincing proof. He discusses the variations in the intensity of these capacities under the influence of narcotic drugs and stimulants and attempts a classification of his subjects by means of the Rorschach test. The evidence in favor of these concepts appears credible, but his prophecy of a Utopian world based on parapsychological abilities seems unduly visionary. His conclusion that this inherent ability "gives support to the most basic and general of all religious doctrines, namely, that man has a spiritual nature" certainly seems premature in view of the relative lack of existing methods for correlating theological ideologies and scientific factors.

GEORGE WINOKUR

HYPNOTISM COMES OF AGE. *Its Progress from Mesmer to Psychoanalysis.*

By Bernard Wolfe and Raymond Rosenthal. The Bobbs-Merrill Company, Indianapolis and New York. $3.00. 272 pp. 1948.

The development of hypnotism is traced by the author from the primitive era, through the "animal-magnetism" period of Mesmer, to the present practice of hypnoanalysis. The vicissitudes of the art are pre-

sented well, the interesting suggestion being made that Freud's abandonment of hypnosis delayed the development of this promising method. The methods, advantages, theories, and dangers of hypnotism are covered sufficiently to interest the psychologically oriented as well as the "man in the street" to whom the book is addressed. Considerable attention is given to the application of hypnosis in therapy, with due and proper cautions concerning its limitations and advantages.

LOUIS F. CLEARY

HUMAN BIOLOGY

MAN'S WAY *from Cave to Skyscraper.*

By Ralph and Adelin Linton; illustrated by Raine Renshaw. Harper & Brothers, New York and London. $2.50. vi + 185 pp.; ill. 1947.

The dust-jacket of this little book implies that it was written primarily for "young people." Certainly, its style is simple, direct, and extremely lucid. The reviewer's 'teen-aged daughter read it with great enjoyment and profit. In any case, it is a book that can be read with great profit by all age groups. It serves as an excellent introduction to the science of general anthropology, and the lessons that it teaches—without any sermonizing—can be of great help in this troubled world.

The broad scope of the book indicated by the chapter headings: Tracing Man's Way; Races of Mankind; The Beginnings of Culture; Basic Inventions; The Coming of the Cities; Islam and India; China and Japan; People of the North; People of the Eastern Ocean; Desert and Jungle; New World; Ancient American Civilizations: and The Last Mutation. The amount of authoritative information that is contained in its less than two hundred small pages is remarkable. Such much abused subjects as race, racial superiority, and the meaning of intelligence tests are dealt with in a dispassionate and illuminating manner. The illustrations by Raine Renshaw are most attractive.

WILLIAM L. STRAUS, JR.

MIRROR FOR MAN. *The Relation of Anthropology to Modern Life.*

By Clyde Kluckhohn. Whittlesey House, McGraw-Hill Book Company, New York and Toronto. $3.75. xiv + 313 pp. 1949.

"Anthropology holds up a great mirror to man, and lets him look at himself in his infinite variety." Anthropology's message of the cultural and biological variability of man is indeed a very important one, and Clyde Kluckhohn has formulated it so ably that this book obtained the Whittlesey House-Science Illustrated Science Fellowship Award, "as contributing the most to man's understanding of the world today."

After a somewhat flippant sketch of the beginnings of anthropology, the author introduces the reader in a very fine chapter (II) into the basic concept of cultural anthropology: "culture." Chapters on Archeology, Physical Anthropology and Linguistics (III, IV, and VI) are excellent and very readable expositions of the basic meaning of these three other foundations of modern anthropology. The chapter on Race: A Modern Myth (V) deserves particular praise for its poise, clarity, and practical suggestions. The chapter on Anthropologists at Work (VII) is primarily a somewhat uncritical description of the war contribution of Allied anthropologists. Personality in Culture (VIII) is far more balanced than the utterances of many other contemporary anthropologists on their favorite subject, the influence of child-rearing on culture, but in the reviewer's opinion is still too full of unproven psychoanalytic mumbo-jumbo. In the chapter An Anthropologist Looks at the United States (IX) we find the first serious contribution of an anthropologist to an analysis of contemporary American culture. An Anthropologist Looks at the World (X) is a rather vague plea for international understanding.

The book is the fruit of great learning, and the author has sprinkled it liberally with very well chosen quotations. The book is equally full of fine and original ideas, and the author has sometimes a very happy hand at making formulations like the term "serial monogamy" for our present-day marriage system. The book is thus very worth reading. Whether it will always be easy reading for the "educated layman" is open to doubt. Chapters like II, VIII, or IX lack the clarity of arrangement and concreteness of exposition that are characteristics of a "popular book." The very virtues of the author as a conscientious scholar and theoretician become here, in a way, his handicaps.

Besides praise the book deserves one serious criticism. The author opens his book with the remark: "Of course, no branch of knowledge constitutes a cure-all for the ills of mankind. If any statement in this book seems to support such messianic interpretations, put this absurd claim down as a slip of an enthusiast who really knows better." Unfortunately the book is so full of such slips that they are one of its characteristics. The author, for example, claims repeatedly that anthropology can predict the behavior of people (e.g., p. 39), or that "anthropology can lay bare the internal logics of each culture" (p. 262). As a fellow-anthropologist I am sorry but also obliged to say that in most cases it cannot. That is obvious in view of the fact that certain "...of the best established generalisations of social anthropology," e.g., "that when the pressure of whites upon aboriginals reaches a certain point there will be a revival of the ancient religion or a partially new cult of messianic type will arise" (p. 66) or that "no people are ruled by women" (p. 2), are not well

established at all. (Not to mention the silly books on "national character" by anthopologists that I had to review for this journal during the last few years!) The author likes to talk of the anthropologist as the "social doctor." I am willing to accept the term with the qualification: "doctor of the 16th century." It is well known that many patients fared better when left alone by the early adepts of modern medical science, although on the other hand there would have been no modern medicine or its triumphs without those men.

In spite of the horrors of the "national character" books mentioned above, the author insists on the ability of anthropologists who can deal with simple preliterate societies to deal also with complex literate civilisations. I should like to answer him with a statement of his own which is fortunately true for only a certain number of anthropologists: "Compared with the average sociologist he [the anthropologist] is almost illiterate" (p. 1). And even the sociologists, and for that matter no single branch of the social sciences, could do it alone.

A characteristic symptom of the author's "messianism" is to claim as inventions of anthropologists what is not theirs, like the "indirect rule" of British colonial administrators (after all, more than four hundred years old); the relation of certain mental diseases with certain constitutional types (found by the psychiatrist E. Kretschmer in 1921); the statistical treatment of medical data (introduced by P. Louis, 1829); the follies of modern baby training (an "accomplishment" of the psychoanalysts); etc. Kluckhohn overlooks the fact that his analysis of American culture is (fortunately) not based on anthropological concepts, but really on the "frontier" concept of the historian, Frederick Jackson Turner. (Most anthropologists after Malinowski repress successfully the fact that history is no less a "mirror of man's variety" than anthropology.) And even before the arrival of anthropology it was unfortunately known to only too many "how important it is whether one's first friends in the society being investigated are placed toward the top or toward the bottom of that society."

Clyde Kluckhohn's book thus mirrors not only the splendor, but also some of the miseries of present-day American anthropology, which has become in part a war victim. During the war a great many anthropologists left for the first time their "ivory towers," did "practical" and often very useful work for government agencies (and were for the first time well paid, "powerful," and "respectable"). Unable to see the accidental nature of most of their usefulness, and unwilling to give up the satisfaction of "being useful," they have adopted the "social worker's" attitude, of which the author finds so many motes in his sociological brethren's eyes. At a moment when our society as a whole was fortunately becoming at long last more conscious of the value of pure science the anthropologists have developed a contempt for pure science, and somehow have lost sight of the developmental stage and limitations of their own science. We hope that this, like other war wounds, will soon be healed.

ERWIN H. ACKERKNECHT

MAN, THE UNKNOWN. *Pelican Books. By Alexis Carrel. Penguin Books, West Drayton, Middlesex.* 1s. 6d. (paper). 312 pp. [1935]; 1948.

Penguin Books have reprinted many worthwhile books, but this is not among them. That a noted scientist could hold such unsound biological ideas and turn them to such pernicious ends is truly ground for consternation. The chief value of this book is that it will reveal to the reader why Carrel was a Nazi sympathizer and from what source Lindbergh became infected. The book is seeded with the virus of racial superiority. It is also a ludicrous, if pathetic, spectacle to see the diminutive Dr. Carrell, a victim of his inferiority complex, stoutly asserting at frequent intervals the lack of correlation between stature and a well-developed physique, on the one hand, and superior intelligence, on the other.

BENTLEY GLASS

ABOUT THE KINSEY REPORT. *Observations by Eleven Experts on "Sexual Behavior in the Human Male." Edited by Donald Porter Geddes and Enid Curie. A Signet Special, The New American Library, New York.* 25 cents (paper). 168 pp.; ill. 1948.

This collection of wholly favorable essays on the Kinsey Report includes the following: New Light on Sexual Knowledge (Geddes); Sexual Behavior Among Primitive Peoples (Clellan Ford); Sex Is God-Given (Frederick C. Kuether); Sex and Character (Erich Fromm); Understanding Our Sexual Desires (M. F. Ashley Montagu); Sex on the Campus (G. M. Gilbert); Sex and Social Attitudes (Robert M. MacIver); Sex and Human Love (O. Spurgeon English); The Limits of Sexual Law (Karl N. Llewellyn); Sex and Class Behavior (Eli Ginzberg); Who Educates Our Children? (Benjamin C. Gruenberg); The End of "Hush and Pretend" (Robert L. Dickinson). Among these, the discussion of the relation of law to sex behavior by Llewellyn may be singled out as particularly penetrating and well balanced. Ginzberg, too, is very stimulating when he correlates the sex behavior patterns of different classes in regard to postponement during the years of preparation or current gratification, with the economic levels of the classes. The general level of all the discussions is on a worthwhile plane.

BENTLEY GLASS

ARCHIVES ETHNOS. *English Edition. Number 1, May, 1948.*

Edited by W. A. Ruysch. Archivos Etbnos, Buenos Aires. Arg. $2.56; US $0.64; subscription, US $2.50. 32 pp.; ill.

This most unusual form of scientific journal contains three parts, or "Series" as they are called: Series A consists of lithoprinted and illustrated abstracts, each a page or more in length, and covering papers published from 1900 to 1948, presumably picked out on the basis of significance rather than current date. Series B, called Collaborations, is printed, and contains one paper, Cord Impression Decoration in Chaco Ceramics, by Stig Ryden. Series C, entitled Biobibliographies, contains a portrait, brief biography, and complete bibliography of the great anthropologist Erland Nordenskiold (1877–1932). To non-Spanish-speaking people interested in the anthropology of the South American Indians, this journal will be of interest. The cost is prorated at the rate of .08 Argentine pesos, or 2 U. S. cents, per page.

PLENTY OF PEOPLE. *The World's Population Pressures, Problems, and Policies, and How They Concern Us. Revised Edition.*

By Warren S. Thompson. The Ronald Press Company, New York. $3.50. xiv + 281 pp.; text ill. 1948.

Warren Thompson, Director of the Scripps Foundation for Research in Population Problems, requires little introduction to biologists. Those who are not familiar with the work of this scholar will find an excellent opportunity to become so in the revised edition of *Plenty of People.* For years he has taken a leading role in analyzing the factors that influence population growth. In this book, written for the intelligent layman, he presents some of the questions arising from the differential in population growth and distribution throughout the world. In addition, Thompson has something to say concerning the implications of present trends and he says it with moderation. His clear and honest treatment of the data is, in itself, a recommendation, particularly since it comes at a time when there are many "scare" and sometimes even hysterical treatises dealing with similar problems. He has interpreted in a rational manner present and future social, political, and economic effects of the birth and death rates in all parts of the world.

Probably few readers of this journal are unaware of the current discussions of the problems of population growth, food supply, and the general welfare. Certainly here in the United States, even though famine has touched us only in the imagination and not in the stomach, no intelligent person is ignorant of the effects of uncontrolled and rapid population growth, such as has existed in China, India, and other Asiatic lands. In such countries the only hope of obtaining a better than meager standard of living is through rigorous measures of birth control that will keep the birth rates at about one-third of the physiological maximum. The only other alternative is the Malthusian dilemma of increasing death rates due to famine and disease. These are the natural, though unwelcome, controls on numbers that are constantly operating in countries where population tends to grow faster than food supply. A rather recent dramatic example occurred in India during the famine and influenza epidemic of 1918 which, according to Thompson, must have taken a toll of 22 to 23 million people. Men of good will naturally wish to avoid such unwelcome corrective checks.

On the other hand, in Western nations, including this country, the problem is not one of too rapid population growth but of developing a population policy that will, among other things, stay the decline in the size of the population. For instance, Thompson points out that by 1975 the U. S. A. will have reached a population of about 165 million people, and the data indicate that, unless some policy is adopted to encourage at least a maintenance birth rate, the population will then start to decline. The social, political, and economic effects of such a decline in population can be as far-reaching, though doubtless not as dramatic, as too rapid a growth. The author believes that England, Germany, and Italy have more people than is desirable, from the standpoint of providing them with a good living, while Australia, New Zealand, and Brazil can stand a fairly rapid increase of population. The outlook for China, Japan, India, and other Asiatic countries is most distressing until they can learn to control their birth rates.

Population policies, for the most part, are concerned with controlling numbers, but it is apparent that in order to provide a decent life for all mankind national population policies should make a "*satisfactory adjustment* of numbers to resources," as well as take 'into account the different values which give life meaning to different peoples."

Space does not permit a full review of this edition. The reader is referred to a review of the first edition (QRB 19: 359, 1944), which deals with other parts of this book. On the basis of its clear and interesting style and the care and fairness used in presenting, analyzing, and interpreting data, this book is highly recommended. It should provide very stimulating reading.

DAVID B. TYLER

EXPERIMENTAL DESIGNS IN SOCIOLOGICAL RESEARCH.

By F. Stuart Chapin. Harper & Brothers, New York and London. $3.00. xii + 206 pp.; ill. 1947.

This small book was written to demonstrate the fact that controlled experiments can be done in sociological experimentation. After an introduction that discusses the general problems of controlled experimentation, 'three basic experimental designs are described and later illustrated from the sociological literature. The first design is the cross-sectional experiment, in which a

control group is compared to an experimental group, with data obtained at one time. The second design is the projected experiment, in which differences are noted between present and future measurements, either with or without a control group. The third design is that in which a present measured effect is traced back to assumed causes, again either with or without the control group comparison. Throughout the illustrations, great emphasis is placed on the necessity for controlling variables which may affect the results, but which in themselves are not objects of study. The paired individual and paired group techniques are discussed in this respect.

The general treatment of experimental design is clear and instructional. On the other hand, the techniques discussed are fairly elementary, particularly when one considers the methods of analysis of variance and covariance. Several of the illustrations used, for example, would have been greatly improved experiments if these techniques had been used. While this book serves as a good introduction to experimental method in sociology, the sociologist who stops here will not be able to use the precision which is possible with modern methods.

W. R. GARNER

BIOMETRY

EXPERIMENT DESIGN AND JUDGMENT OF EVIDENCE. *By Errett C. Albritton. Published by the Author, George Washington University, School of Medicine, Washington, D. C.* $2.75 (paper). viii + 114 pp.; ill. 1948.

This is a highly interesting manual with a worthwhile aim. It attempts to teach medical students the logic underlying the use of the experimental method in clinical medicine. Thus, although it must deal with statistical methods, it is not a textbook of statistics. It considers such topics as correlation and causation, the procedures for designing an experiment to investigate the effect of one variable on another, the necessity for having "control" observations, and the techniques by which experimental results can be evaluated statistically. One chapter contains a series of carefully worked out, model experiments so that the researcher can select a valid procedure for his own purposes. Another chapter applies the principles to the evaluation of experimental data reported in the *Journal of the American Medical Association* and other medical journals.

Despite its worthwhile aim, this manual has some serious deficiencies. The author is careful to point out that correlation, in a statistical sense, does not imply causation, but he then goes on in the rest of the manual to talk about agent A "causing" a change in X. The extremely important point about how an investigator can validly infer causation from an experiment is treated much too glibly to satisfy a meticulous reader.

In some places, too, the author is wrong—dangerously wrong, considering the aim of the manual. In discussing differences in the variability of data, for example, he presents a chart showing two hypothetical sets of blood pressure readings. About this graph he says, "In the succeeding graph it is possible again to reject the null hypotheses *on sight* [the emphasis is the reviewer's]. . .without estimating ½ P for the difference in variability, we judge that the variability in the second group is significantly greater than that in the first. . ." Actual computation shows that the difference is barely significant. Now this guesswork is dangerous because the author is attempting to teach the student to avoid precisely this kind of uncritical judgment.

In another place Albritton talks about the calculation of chi-square for cell frequencies of 8 and 2. This is just the kind of situation for which chi-square has questionable validity. He discusses the calculation of a coefficient of variation without pointing out an important limitation that is a matter partly of logic and partly of statistics, namely, that the coefficient of variation is valid only when the scale in which the event is measured has the properties of a ratio scale.

The author has undertaken an extremely important mission. Since the manual is apparently undergoing yearly revision, perhaps the next edition will contain the precision that is so necessary to this topic.

A. CHAPANIS

INTRODUCTION À LA BIOLOGIE QUANTITATIVE. *Présentation et Interprétation Statistique des Données Numériques.*

By M. Lamotte; preface by Georges Darmois. Masson et Cie., Paris. 1500 fr. (paper). vi + 370 pp.; ill. 1948.

This is a text on statistical methods applied to biological problems. It has two main sections: Part I (12 chapters) covers the descriptive statistics; Part II (11 chapters) covers sampling statistics. The reader will find here most of the topics covered in elementary textbooks plus several not ordinarily found in comparable treatises. Although there is a chapter on the analysis of variance, the author's approach does not place as much emphasis on this topic as do the textbooks by Fisher or Snedecor. Small sampling statistics are otherwise emphasized.

The author's exposition is careful and thorough, yet it is likely that most biology students would find it difficult reading. For this reason, the textbook would be most appropriate for a full-year course in the subject. In the reviewer's opinion, this book ranks with the best in its field and, were it written in English, it would undoubtedly be a popular collegiate textbook in this country.

A. CHAPANIS

ELEMENTARY STATISTICAL ANALYSIS.

By S. S. Wilks. Princeton University Press, Princeton; Geoffrey Cumberlege, Oxford University Press, London. $3.75 (paper). xii + 284 pp.; ill. 1948.

A large number of statistics textbooks seems to have appeared in the last few years. For the most part, they all cover roughly the same content but differ in approach. This one, designed for a one-semester course, differs from most in that it is characterized by heavy emphasis upon sampling theory and techniques. Long before the student has read a quarter of the way through the book, he is deep in a discussion of probability. This is a stiffer dose of probability and sampling than one finds in most elementary books.

Wilks' approach has much to commend it, and a choice among textbooks depends ultimately on what individual instructors consider important. This one, as might be expected, is light on the descriptive statistics. The mean is the only measure of central tendency discussed, the standard deviation the only measure of variability. Kurtosis and skewness are not mentioned. But the chapter on the Poisson distribution is one of the best the reviewer has seen in any textbook, elementary or advanced.

The exercises are generally suitable for students in the biological sciences. Although the author claims that the text "presupposes one semester of elementary mathematical analysis," the book requires a better mathematical background than many others at a comparable level.

A. CHAPANIS

PSYCHOLOGICAL STATISTICS.

By Quinn McNemar. John Wiley & Sons, New York; Chapman & Hall, London. $4.50. viii + 364 pp.; ill. 1949.

The subject matter of statistics has become so complex in recent years that each of the sciences which uses statistical tools has had to develop its own specialized brand. The title of this book is appropriate, since it is indeed a book on psychological statistics. It has included and emphasized those tools which are most useful to psychologists, and has spent little time on the many interesting, but to psychologists relatively useless statistics which are so frequently included in statistical textbooks. After an introduction, a chapter on tables and graphs, and a chapter on descriptive statistics, there is a full chapter on the meaning of the normal curve and probability, plus another chapter on sampling errors and statistical inference. There are then five chapters on correlational methods, including partial, multiple, biserial, tetrachoric, and fourfold point correlations, contingency coefficients, and the correlation ratio. A chapter on Chi square, and one on small sample methods, are followed by three chapters on the analysis of variance, and a last chapter on sampling techniques. There has been deliberate omission of factor analysis techniques, which is the only major technique used by psychologists not included in this book. Tables of normal curve functions, transformation of r to z and z to r, Chi square, t and F are appended. The sequence of topics seems good, except that the chapter on small sample methods should have come earlier in the book. It could have followed the chapter on sampling errors with no break in thought. On the other hand, the discussion of probability and sampling errors early in the book is excellent, and should give the student a far better idea why he is learning some of the later techniques.

There are many good things to be said about this book. Throughout the book, the use of statistics as a tool in research is emphasized, and many times the importance of the original experimental design is stressed. An entire chapter is spent on the interpretations of correlation coefficients, and on the assumptions which are made. Quite extensive sections are spent on the meaning of the analysis of variance, and possible interpretations. This emphasis is exceptionally good, and should give the student the proper perspective toward statistics.

Derivations are included only when the author thinks that they lend to the understanding of the concept. Actually, there are plenty of derivations for the beginning and intermediate student; and in general it seems that the number of derivations is sufficient except for one. The use of degrees of freedom to offset the bias in estimating sigma is given to the student to accept on faith. The use of degrees of freedom in small samples is so universal and basic a procedure today, however, that the student should have a complete understanding of its purpose. It is, of course, central to all of the techniques used in the analysis of variance, and must be completely understood. In fact, the only chapter which seems inadequate is this one chapter on small sample methods.

The notation is simple, clear, and consistent. The student should have little trouble following it from beginning to end. Where a new notation is introduced, it is adequately explained as soon as it is used, which leads to a minimum of confusion. The material is extremely condensed, and it is not clear that the book will be useful for all courses in statistics. The ideal use of the book would be for a continuous one-year course. It could also be used as textbook in a second-semester course, but the material is probably too condensed for most first-semester courses. Less than 200 pages are used, for example, to cover all forms of correlation. Nevertheless, in the ideal course this would be the ideal textbook. Everything useful is in the book, and all is clearly explained.

In short, this is probably the best single book on psychological statistics available, and most psychologists will want it on their shelves. It is complete enough to be an excellent reference, particularly on the

analysis of variance—a subject poorly handled in most books. The statistical problems of the biological scientist are similar enough to those of the psychologist that most of them will also want it.

W. R. GARNER

MACHINE COMPUTATION OF ELEMENTARY STATISTICS. *With Special Reference to the Friden, Marchant, and Monroe Calculating Machines.*

By Katharine Pease. Chartwell House, New York. $2.75 (spiral bound; paper). xii + 208 pp.; ill. 1949.

This manual is for students who are learning to use computing machines in connection with courses in elementary statistics. It is a self-teaching manual, so that the student, by following the procedures in sequence, can learn to use the machines with minimum help from an instructor. Instructions are given in a parallel arrangement for the Friden (Model ST-10), Marchant (Models ACT-10M and ACR-8D), and Monroe (Models CAA, AA-1, MA-7K, and Educator) calculating machines.

The manual starts with the elementary operations of addition and subtraction and ends with techniques for computing means, standard deviations, and product-moment correlations. The instructions are clear and liberally illustrated with examples and problems. The student and investigator who have never learned to use computing machines will find this manual of immense help. It will also be an indispensable teaching aid in any statistical laboratory.

A. CHAPANIS

DE OMNIBUS REBUS ET QUIBUSDAM ALIIS

SCIENCE AT WAR.

By J. G. Crowther and R. Whiddington. Philosophical Library, New York. $6.00. vi + 195 pp. + 44 plates; text ill. 1948.

During the latter days of the war Admiral Doenitz, in a speech at Weimar, is reputed to have said "...the scientists who created radar have been called the saviors of their country. It was not superior strategy or tactics which gave him success in the U-boat war, but superiority in scientific research."

This book deals especially with the scientific contributions of the British. Of the four basic topics covered (radar, the atomic bomb, operational research, science and the sea), Crowther and Whiddington devote over half their book to the first. Using material supplied from official archives and documents of the Scientific Advisory Committee to the British Cabinet, they have written one of the best detailed accounts of science's role in the war effort and have illustrated it with almost a hundred drawings and photographs.

The authors point out that the first job was to "make England an island again." They trace the developmental stages that made possible the surrounding of the east coast of Britain by radio waves capable of detecting planes 100 miles away. As a result of the development of the 10 and 1½ meter radar, day attacks on Britain were beaten; and by improved guiding stations on the ground and better interception sets in the planes, the night battles over Britain were also won. During the first two years of the war 90 per cent of the bombs dropped by British planes fell harmlessly in open fields. By using and developing certain radar techniques, a system of stations was set up that made possible the accurate guidance of large numbers of aircraft and the 1,000 bomber raids. In August, 1940, it took an average of 20,000 rounds to bring down one unseen enemy aircraft. By 1941, the number was reduced to 4,000, and 100 planes were shot down. The development of the cavity magnetron made possible the practical use of the very high frequency waves (10 centimeter waves, with frequencies of about 3,000 million per second). There is ample evidence to substantiate the claim that "the development of this valve had a more decisive effect on the war than any other single weapon."

World War II witnessed the extension of scientific method into wider aspects of warfare. Up to that time science had been chiefly concerned with the improvement of old weapons. In this book it is pointed out that the development of operational research was perhaps the chief scientific feature of the war. The romantic idea of war was outmoded. Hitler, a proponent of the latter conception, was out of date, and contributing to his defeat was the fact that he failed to produce any operational research comparable to the British. The reader cannot fail to be impressed by the extent to which that failure was a result of the nature of fascism and to what measure our success stemmed from the practice of democracy.

DAVID B. TYLER

THE COLOR OF LIFE.

By Arthur G. Abbott. McGraw-Hill Book Company, New York and London. $6.00. xxii + 294 pp. + 7 plates. 1947.

It appears that the intention of this author was to provide in one volume a complete survey of the multiplex study of color in all its ramifications. He begins with the physics of color production and the physiology of wave-length discrimination, with emphasis on cataloging examples of the creation of specific colors, rather than on general principles. By the end of this part of the book the reader is overwhelmed by the obscure complexity of it all, and nowhere in the succeeding

chapters is that effect diminished. The last two-thirds of the book is concerned with the uses of color. This discussion consists chiefly of the listing of rules for color use in specific situations and the citation of examples of successful utilization of color in clothing, in schools and hospitals in Washington, D. C., in signal flags, etc. The author closes with a consideration of color and Man's Progress, which compares the uses of color in times past with some modern usages, or with usages recently suggested.

Little attempt has been made to indicate differences in validity between objective findings and subjective opinions. The author carefully points out that the rhodopsin-vitamin A cycle is only theory (p. 10), but such a peculiar caution is of questionable value in a book wherein the differential effects of colored lights on muscular activity are treated as established fact (p. 129). For the most part, specific references to original sources are lacking, and most of the acknowledged "sources of information" are secondary compilations, ranging in scholarship from *The Encyclopedia Britannica* to Podolsky's *The Doctor Prescribes Color*. It is not that this book is inaccurate. Some spot checks by the reviewer indicate a faithful reporting of the material in the sources. It is simply uncritical.

This book has little value for the scientist working on color research. On the other hand, it could provide a rich source of miscellaneous listings and catalogs of items of color lore, surrounded by a magical aura of scientific profundity, suitable for writers of Sunday magazine supplement articles and imaginative fiction.

HOWARD D. BAKER

MANUAL OF CLINICAL LABORATORY METHODS. *Fourth Edition.*

By Opal E. Hepler; with a foreword by James P. Simonds. Charles C. Thomas, Springfield, Ill. $8.50. xvi + 387 pp.; ill. 1949.

A noteworthy feature of this manual is the explicit and precise description, step by step, of the procedures for the performance of the various tests covering urinalysis, hematology, gastric and duodenal contents, liver functions, feces, sputum, cerebrospinal fluid, body fluids, pregnancy, bacteriology, mycology, serology, blood groups, clinical chemistry, allergy extracts, tissue sectioning, basal metabolism, and electrocardiography. Hepler's style is to be commended and is a unique feature of this manual. The textbook is designed as a guide for the average clinical laboratory technician and medical student and, as such, can be recommended particularly for those in institutions where the choice of methods is not of great consequence and where the methods in general use are those given in the manual. However, the book has some major shortcomings. In order that the laboratory technician or medical student "need not labor over the selection of a method," only one method, in most instances, is given for each determination. This fact, together with the absence of a bibliography, make the manual of limited value to the more critical laboratory worker.

DAVID B. TYLER

MICROFOTOGRAFIA E MACROFOTOGRAFIA. *Principi e Applicazioni della Tecnica Microfotografica al Laboratorio di Ricerche Scientifiche e Industriali.*

By Edgardo Baldi. Ulrico Hoepli, Milano; W. S. Heinman and Company, New York. $6.50 (paper). xii + 349 pp.; ill. 1946.

A comprehensive book that has a refreshing way of presenting old subject matter in a new light. New drawings have been included, and all in all this should remain a standard reference in its field for a long time to come.

CHARLES P. SHILLABER

NATURWISSENSCHAFTLICHE RUNDSCHAU, *Volume I, Number 1. July, 1948. Monthly. Edited by H. W. Frickhinger. Wissenschaftliche Verlagsgesellschaft M. B. H., Stuttgart.* Quarterly subscription, DM 7.20; single issue, DM 2.40; reduction for students (paper).

This new general journal of science, to appear monthly, is most like *Naturwissenschaften* and *Experientia* in character. The first issue, for example, carries the following articles: Das moderne naturwissenschaftliche Weltbild (Paul Walden); Das Werden der Kontinente (S. von Bubnoff); Die Bedeutung aktiver fester Stoffe für Wissenschaft und Technik (R. Fricke); Synthetische Nahrungsmittel (K. G. Bergner); Schmetterlings-Farbstoff als Lebenselement (W. Rudolph); Wie hoch ist der Himmel? (H. Siedentopf); Zum Problem des Golfstroms (Gerhard Neumann); Die pflanzliche Organ- und Gewebekultur (O. Kandler); Forschungsreise in ferne Länder (Hans Krieg). In addition there are death notices of scientists, obituaries, scientific anniversaries, brief reviews of important papers, book reviews, etc. Illustrations are relatively few, and vary considerably in quality of reproduction.

INTRODUCING AUSTRALIA. *Revised Edition.*

By C. Hartley Grattan. The John Day Company, New York. $5.00. xvi + 357 pp. + 12 plates; text ill. 1947.

In this edition, the author has brought his popular exposition (nine impressions since 1941) of Australian life and political economy up to date by extensive rewriting and additions. World War II served to dispel some of the misconceptions common in American minds about the land "down under," and this well-

balanced survey of things as they are should do much, as its preceding edition has undoubtedly done, to extend this clarification. It should serve also to accelerate the development of a realistic outlook about their country in many Australian minds. The hard facts are that, apart from a fertile coastal belt and certain mineral resources, Australia is not potentially another United States. Nevertheless, by virtue of its geographical situation and a conscious eclecticism in social striving, it is developing responsibilities in the Pacific which must be considered seriously in any comprehensive economic or military view. These pertinent facts are couched in direct and readable prose and accompanied with fraternal analysis and criticism of a nation and people the author has willingly taken into his personal care. Unfortunately for the biologist, he will find in these pages little of "the cenotaphs of species dead elsewhere."[1]

D. H. K. LEE

PROPAGATION OF SOUND IN THE OCEAN. (*1*) *Explosion Sounds in Shallow Water.* (*2*) *Theory of Propagation of Explosive Sound in Shallow Water.* (*3*) *Long-Range Sound Transmission. The Geological Society of America Memoir 27. Contribution Number 415 from the Woods Hole Oceanographic Institution.*

(*1*) *By J. Lamar Worzel and Maurice Ewing;* (*2*) *By C. L. Pekeris;* (*3*) *By Maurice Ewing and J. Lamar Worzel. The Geological Society of America, New York.* $2.75. (1) vi + 54 pp. + 8 plates; text ill. (2) viii + 117 pp. + 11 plates; text ill. (3) vi + 36 pp. + 4 plates; text ill. 1948.

RECONNAISSANCE OF THE GEOLOGY AND OIL POSSIBILITIES OF BAJA CALIFORNIA, MEXICO, *The Geological Society of America Memoir 31.*

By Carl H. Beal. The Geological Society of America, New York. $3.00. x + 138 pp. + 9 plates + 2 maps. 1948.

THE QUARTERLY REVIEW OF BIOLOGY publishes critical reviews of recent researches in all of the special fields of biological science. The contribution should present a synthesis or digest of the researches and a critical evaluation of them. A mere synopsis of the literature without evaluation or synthesis is not desirable.

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Material ordinarily taking the form of footnotes are set in small print and placed in the text and consequently should be written in a style so as to fit readily into the text. Acknowledgments are printed in the text in small type at the end of the article just preceding the List of Literature. Recent issues of the Quarterly should be examined for style as regards (1) section or subsection headings in the text, (2) literature citations in the text, and (3) List of Literature.

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